AN

ESSAY ON DEW

AND SEVERAL

APPEARANCES CONNECTED WITH IT,

BY

WILLIAM CHARLES WELLS.

EDITED, WITH ANNOTATIONS,

BY

L. P. CASELLA, F.R.A.S.;

AND AN APPENDIX

By R. STRACHAN, F.M.S.

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LONGMANS, GREEN, READER, AND DYER.
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PREFACE.

A reprint of the celebrated Essay on Dew by Dr. William Charles Wells has long been required by meteorologists and scientific students. The last edition dates from 1821, and copies of the work are now very scarce. The subject of which it treats is exceedingly interesting, and has much scientific importance, while the Essay, as a literary production, is a model worthy of the attentive study of scientific experimentalists and students. It is a lucid exposition of the development of a theory, after the strictly inductive method, from an accumulation of facts which were established by experimental tests.

Sir J. F. W. Herschel, in his "*Discourse on the study of Natural Philosophy*," published in 1830, designates the theory of dew, as developed by Wells, "one of the most beautiful specimens we can call to mind of inductive experimental enquiry lying within a moderate compass. It is not possible in so brief a space to do it justice; but we earnestly recommend his work (a short and very entertaining one) for perusal to the student of natural philosophy, as a model with which he will do well to become familiar."

George Harvey, the author of the treatise on Meteorology, in the *Encyclopedia Metropolitana*, recommended most strongly to the reader's attention, Dr. Wells's *Essay on Dew;* and he added: "We know of no work in our day (writing in 1836) which has been more universally admired than the Treatise of Dr. Wells, certainly none that practically exemplifies in a purer and better form the admirable inductive system which it was the object of Bacon to teach."

Professor John Tyndall, the brilliant experimentalist, whose discoveries relative to the kindred subject, aqueous vapour, have thrown a new light upon meteorology, in the second edition of his work entitled *"Heat considered as a mode of Motion"* says: "A series of experiments, conceived and executed with admirable clearness and skill, enabled Dr. Wells to propound a Theory of Dew, which has stood the test of all subsequent criticism, and is now universally adopted."

And he further eloquently remarks: "With broken health Wells pursued and completed his beautiful investigation; and, on the brink of the grave, he composed his Essay. It is a model of wise enquiry and lucid exposition. He made no haste, but he took no rest till he had mastered his subject, looking steadfastly into it until it became transparent to his gaze. Thus he solved his problem, and stated its solution in a fashion which renders his work imperishable."

In contrast with this unqualified praise, coming from men so eminent in science, neither prompted by friendship, nor uttered for vindication of opinion, but given as the genuine expression of unbiassed judgment, notwithstanding the lapse of half of a century, let us, in fairness, place before the reader a criticism by a writer who has done good service by his enquiry into the history of the subject. Professor Charles Tomlinson in his essay entitled *"History of the Modern Theory of Dew,"* has fairly shown that the phenomena of Dew had been observed and experimented upon by Boyle, Le Roi, Pictet, Alexander and Patrick Wilson, Six, Prevost, and Young, before Wells; and memoirs on the subject had been published prior to the appearance of Wells's essay. To the question: What then remains for Wells? He answers thus: "To Wells belongs the rare merit of seeing clearly where other men saw obscurely; of grasping the whole while other men held only detached parts; of bringing the scattered and somewhat incoherent labours of other inquirers to bear upon his own experiments, which were undertaken with clearer views, and consequently a more direct purpose than those of his predecessors; and the final result of his long and patient inquiry was the establishment of a theory of extreme beauty and simplicity, the truth of which subsequent inquiry has only tended to confirm; not that Dr. Wells is to be ranked as the author of this theory, but that his essay was, as Dr. Whewell remarks,* 'one of the books which drew most attention to the true doctrine in this country at least.'" The legitimacy of this conclusion may well be questioned; for, granting that all the main facts and features of the theory were known beforehand, if Wells took and built them up into a theory, he most certainly is fully entitled to be called the author of the theory. That he insufficiently acknowledged the labours of his predecessors may be admitted; and, in this edition, an endeavour is made in the notes to supply the most important omissions. The notes thus introduced are distinguished by numbers running consecutively.

In the preparation of these notes, the works of the following authors have been consulted:—Daniell, Harvey, Herschel, Glaisher, and Tomlinson, while Tyndall has been carefully studied as the latest discoverer in this branch of physics.

Not only are the labours of the predecessors of Wells noticed, but the researches of his successors are also related; so that, as well (it is hoped) as this mode of treatment allows, the present state of our knowledge of the subject is presented in a complete form.

A few sentences will suffice for such information regarding Dr. Wells as modern readers will generally care to have. He dictated a succint memoir of his life, a short time only before his death, which was prefixed to the edition of his two Essays, on Vision and on Dew, published in 1818. From this memoir our account has been drawn.

William Charles Wells was born in Charlestown, South Carolina, in May, 1757. He was sent to a school in Dumfries before he was eleven, and in 1770 entered Edinburgh University. The next year found him at Charlestown, apprenticed to a doctor. In the disputes between the colonies and the mother country, he espoused the Royalist cause, and in 1775

* "*History of the Inductive Sciences,*" 3rd Edition, vol. II., 1857.

found it necessary to leave America. He studied at Edinburgh and afterwards in London, for the medical profession. In 1781 he returned to America; and, while on a coasting voyage, he suffered shipwreck, but saved himself by swimming to shore. In 1784 he returned to London, and commenced practising as a physician, although he had abandoned his profession while in America. For many years he struggled hard and earned little, but eventually he stood well in his profession and made a good income.

In 1812 he commenced an enquiry into the nature of dew, which, with failing health, he continued in 1813 and 1814. His physical condition then became so serious that he was advised to desist from his experiments, which necessarily exposed him to the cold night air, as it was thought improbable he could survive more than a few months. "Upon receiving this opinion," he himself says, "I set about immediately composing my Essay on Dew, as my papers containing the facts on which my theory was founded would, after my death, be altogether unintelligible to any person who should look into them. I laboured, in consequence, for several months with the greatest eagerness and assiduity, fancying that every page I wrote was something gained from oblivion." The Essay was published in August, 1814, and he survived till September 18th, 1817.

Dr. Wells was admitted into the Royal Society of London in 1793, and into that of Edinburgh in 1814. His writings on medical, philosophical and biographical subjects were very numerous. At the present day the Essay on Dew is the only one of them which can lay claim to the attention of the general reader. A list of his writings is given in the edition of his Essays on Vision, and on Dew, published in 1818, and reprinted in 1821.

TO

JAMES DUNSMURE, ESQUIRE.

MERCHANT IN LONDON.

My Dear Sir,

Without your aid, I should, in all probability, never have acquired the knowledge, upon which the following Essay is principally grounded; since I could not, I believe, have found any other place, considering that I was obliged to be daily in London, so well fitted for my experiments, as that which you permitted me to use during a very long time, though manifestly to the great inconvenience of yourself and your family. I beg leave to assure you, that I feel this kindness most strongly, and that my gratitude for it will never cease to exist.

I am,

My dear Sir,

Your most obedient Servant,

and faithful Friend,

WILLIAM CHARLES WELLS.

London, August, 25, 1814.

The following notice was prefixed by the Author to the second edition, published in 1815.

The infirm state of the Author's health having prevented him, since the publication of the former edition of his Essay on Dew, from making experiments in the open air during the night, and his reading having in the meanwhile been directed to other objects, the present edition of that Essay will be found to contain almost nothing more than the other. The chief difference between the two arises from a change in the form of several of his expressions. He has, for instance, altered the expression of "saturation with moisture" to that of "repletion with moisture," in order to avoid the appearance of maintaining, that common air is capable of dissolving water; a tenet unconnected with his theory. Sometimes he has subjoined to the phrase, which he now employs, on that subject, the words "in a pellucid state;" when this addition has not been made, he wishes it to be understood.

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INTRODUCTION.

I WAS led, in the autumn of 1784, by the event of a rude experiment, to think it probable that the formation of dew is attended with the production of cold. In 1788, a paper on hoarfrost, by Mr. Patrick Wilson[1] of Glasgow, was published in the first volume of the Transactions of the Royal Society of Edinburgh, by which it appeared that this opinion had been entertained by that gentleman before it had occurred to myself. In the course of the same year, Mr. Six of Canterbury mentioned in a paper communicated to the Royal Society, that, on clear and dewy nights, he always found the mercury lower in a thermometer laid upon the ground in a meadow in his neighbourhood, than it was in a similar thermometer suspended in the air, six feet above the former; and that, upon one night, the difference amounted to 5° of Fahrenheit's scale. Mr. Six, however, did not suppose, agreeably to the opinion of Mr. Wilson and myself, that the cold was occasioned by the formation of dew; but imagined that it proceeded partly from the low temperature of the air, through which the dew, already formed in the atmosphere,

[1] Wilson had published a paper on the same subject as early as 1780, in the Transactions of the Royal Society; and a second paper in the Philosophical Transactions for 1781.

had descended, and partly from the evaporation of moisture from the ground on which his thermometer had been placed. The conjecture of Mr. Wilson and the observations of Mr. Six, together with many facts which I afterwards learned in the course of reading, strengthened my opinion; but I made no attempt, before the autumn of 1811, to ascertain by experiment if it were just, though it had, in the meantime, almost daily occurred to my thoughts. Happening, in that season, to be in the country on a clear and calm night, I laid a thermometer upon grass wet with dew, and suspended a second in the air, two feet above the other. An hour afterwards, the thermometer on the grass was found to be 8° lower, by Fahrenheit's division, than the one in the air. Similar results having been obtained from several similar experiments made during the same autumn, I determined, in the next spring, to prosecute the subject with some degree of steadiness, and with this view went frequently to the house of one of my friends in Surrey. At the end of two months, I fancied that I had collected information worthy of being published; but fortunately, while preparing an account of it, I met, by accident, with a small posthumous work [2] of Mr. Six, printed at Canterbury in 1794, in which are related differences observed on dewy nights, between thermometers placed upon grass, and others in the air, that are much greater than those mentioned in the paper presented by him

[2] The work is entitled: "The Construction and Use of a Thermometer, for showing the extremes of Temperature in the Atmosphere during the observer's absence, together with experiments on the variations of local heat, and other meteorological observations. By James Six, Esq., F.R.S., Maidstone, 1794."

to the Royal Society in 1788. In this work, too, the cold of the grass is attributed, in agreement with the opinion of Mr. Wilson, altogether to the dew deposited upon it. The value of my own observations appearing to me now much diminished, though they embraced many points left untouched by Mr. Six, I gave up my intention of making them known. Shortly after, however, upon considering the subject more closely, I began to suspect that Mr. Wilson, Mr. Six, and myself had all committed an error in regarding the cold which accompanies dew as an effect of the formation of that fluid. I, therefore, resumed my experiments, and having, by means of them, I think, not only established the justness of my suspicion, but ascertained the real cause both of dew and of several other natural appearances which have hitherto received no sufficient explanation, I venture now to submit to the consideration of the learned an account of some of my labours, without regard to the order of time in which they were performed, and of various conclusions which may be drawn from them, mixed with facts and opinions already published by others.

PART I.

OF THE PHENOMENA OF DEW.

SECTION I.

Of Circumstances which influence the Production of Dew.

ARISTOTLE* and many other writers have remarked, that dew appears only on calm and serene nights. The justness of this observation, however, has not been universally admitted. For Musschenbroek† says, that dew forms in Holland while the surface of the country is covered with a low mist; but, as he mentions at the same time that it is deposited upon all bodies indiscriminately, the moisture of which he speaks cannot properly be called dew, as will be more distinctly seen hereafter. Other writers of considerable reputation have also regarded clearness of the atmosphere as not being requisite for the production of dew, misled, I believe, partly by theory, and partly by observing on misty mornings copious dews, which had been produced during *preceding clear nights.* Respecting this point I can aver, after much experience, that I never knew dew to be abundant except in serene weather. In regard to the necessity of the air being still, I know of no person who rejects it, except M. Prieur,‡ a late French author of little consideration, and he affirms, in opposition to the most common observation, that a fresh wind is requisite for the production of dew.

The remark of Aristotle, however, is not to be received in its strictest sense, as I have frequently found a small

* Meteor. Lib. I. c. x. et De Mundo, c. iii.

† Nat. Phil. T. ii. De Rore.

‡ Journal de l'Ecole Polytechnique, Tom. ii. 409.

quantity of dew on grass, both on windy nights, if the sky was clear or nearly so, and on cloudy nights, if there was no wind. If, indeed, the clouds were high and the weather calm, I have sometimes seen on grass, though the sky was entirely hidden, no very inconsiderable quantity of dew. Again, according to my observation, entire stillness of the atmosphere is so far from being necessary for the formation of this fluid, that its quantity has seemed to me to *be increased by a very gentle motion in the air.* Dew, however, has never been seen by me on nights both cloudy and windy.

If, in the course of the night, the weather, from being calm and serene, should become windy and cloudy, not only will dew cease to form, but that which has formed will either disappear or diminish considerably.

In calm weather, if the sky be partially covered with clouds, more dew will appear than if it were entirely covered, but less than if it were entirely clear.

Dew probably begins in this country to appear upon grass, in places shaded from the sun, during clear and calm weather, soon after the heat of the atmosphere has declined.[3] My opportunities, however, for making such observations have not been numerous, since, while pursuing this subject, I seldom went into the country till late in the afternoon; but I have frequently felt grass moist in dry weather, several hours before sunset. On the other hand, I have scarcely ever known dew to be present in such quantity upon grass, as to exhibit visible drops, before the sun was very near the horizon, or to be very copious, till some time after sunset. It also continues to form, in shaded places, after sunrise; but the interval between sunrise and its ceasing to form, is,

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[3] The invisible vapour contained in the superincumbent air, although being a powerful radiant, is not so quickly chilled as the grass, because it has not only to discharge its own heat, but also that of the large mass of air in which it is suspended. Hence the condensation of the vapour in contact with the grass and the formation of dew.

according to my observation, which, upon this point, has not been extensive, considerably shorter than that between its first appearance in the afternoon and sunset. Contrary, however, to what happens at sunset, if the weather be favourable, more dew forms a little before, and, in shaded places, a little after sunrise, than at any other time. Musschenbroek, therefore, errs greatly when he says, that dew does not form after the sun has risen. The preceding observations, on the early appearance of dew in the afternoon, are to be restricted to what happens to grass or other substances highly attractive of dew placed on the ground; for it occurs much later on similar substances, which are elevated a few feet above the ground, though upon these it continues to form as long after the rising of the sun, as upon the others, if they be equally sheltered from the rays of that body.

The formation of dew after it has once commenced, continues during the whole night, if the weather remain still and serene. M. Prieur, indeed, of whom I have already spoken, asserts, that dew forms only in the evening and morning, and that any which occurs in the former season always disappears in the course of the night. I can affirm, however, from long experience, that grass, after having been dewed in the evening, is never found dry until after sunrise, unless the weather has, in the mean time, changed. Upon one serene and still night, I placed fresh parcels of wool upon grass every hour, and by weighing each of them, after exposure for an hour, found that they had all attracted dew.[4]

When dew forms upon a smooth dense body, as glass, and it is only by means of such a body that the process can be

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[4] "To measure the quantity of dew deposited each night, an instrument is used called a *drosometer*. The most simple process consists in exposing to the open air bodies whose exact weight is known, and then weighing them afresh after they are covered with dew."—Kœmtz's *Course of Meteorology*.

accurately observed, the appearances are altogether similar to those which occur on a like body, when exposed to the steam of water a little warmer than itself. The exposed surface has first its lustre diminished by a slight damp uniformly spread over it. As the moisture increases it gathers into irregularly shaped flat drops, which are, at first, very small, but afterwards enlarge and run into one another, forming streamlets, by means of which a great part escapes from the body which had received it.

During nights that are equally clear and calm, dew often appears in very unequal quantities, even after allowance has been made for any difference in their lengths. One great source of these differences is very obvious. For it being manifest, whatever theory be adopted concerning the immediate cause of dew, that the more replete the atmosphere is with moisture previously to the operation of that cause, the more copious will the precipitation of water be; after this operation has commenced, all the circumstances, which tend to increase the quantity of moisture in the atmosphere, must likewise tend to increase the production of dew. Thus dew, in equally calm and clear nights, is more abundant shortly after rain than during a long tract of dry weather. It is more abundant, also, throughout Europe, with perhaps a few exceptions, and in some parts of Asia and Africa, during southerly and westerly winds, than during those which blow from the north and the east. Aristotle* says, that Pontus is the only country in which dew is more copious during a northerly than during a southerly wind. But a similar fact occurs in Egypt; for dew is scarcely ever observed there, except while the Etesian winds prevail. Both cases, however, though contrary to the letter, are consonant with the spirit of the rule; since the north wind, in one country, proceeds from the Euxine sea, and, in the other, from the Mediterranean.

* Meteor. Lib. l. c. x.

Another circumstance of the same kind with the blowing of wind from the south and the west, as showing that the air contains much moisture, is the lessening of the weight of the atmosphere. My experience on this point has not, indeed, been great, as the falling of the mercury in the barometer is very commonly attended with wind or clouds, both unfavourable to the production of dew; but still the greatest dew I have ever witnessed, occurred while the barometer was sinking. A corresponding observation is made by M. de Luc, who says, that rain may be foretold when dew is uncommonly abundant, in relation to the climate and season.* [5]

To the greater or less quantity of moisture in the atmosphere, at the time of the action of the immediate cause of dew, are likewise to be referred several other facts respecting its copiousness, the explanation of which is, perhaps, not so apparent as in the preceding examples.

In the first place, dew is commonly more plentiful in spring and autumn than in summer; the reason is, that a greater difference is generally found between the temperatures of the day and the night, in the former seasons of the year, than in the latter. In spring this circumstance is prevented often from having a considerable effect by the opposite influence of northerly and easterly winds; but, during still and serene nights in autumn, dew is almost always highly abundant.[6]

In the second place, dew is always very copious on those clear and calm nights which are followed by misty or foggy mornings; the turbidness of the air in the morning showing

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* Rech. sur. les Mod. de l'Atm. §725.

[5] And Harvey says:—"In Devonshire, three successive frosty mornings are regarded by the farmer as a sure harbinger of rain."

[6] "It was found that the differences between the temperature of the air and of bodies on the earth, at night, in equally calm and clear weather

that it must have contained, during the preceding night, a considerable quantity of moisture.

Thirdly, I have observed dew to be unusually plentiful on a clear morning which had succeeded a cloudy night. For the air, having in the course of the night lost little or no moisture, was in the morning more charged with watery vapour than it would have been if the night had also been clear.

Fourthly, heat of the atmosphere, if other circumstances are favourable, which, according to my experience, they seldom are in this country, occasions a great formation of dew. For, as the power of the air to retain watery vapour in a pellucid state, increases considerably faster while its temperature is rising than in proportion to the heat acquired,[7] a decrease of its heat, in any small given quantity,

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was the same at every period of the year, but it was found that the amount of dew deposited during such times was much greater in summer than in winter. This is easily accounted for, from the now well-known relation existing between temperature and moisture. At all seasons of the year, at night, the depression of the temperature of the dew-point below that of the air is small, or the air is in a state of saturation nearly, and, therefore, in summer, a certain diminution of temperature would cause much more vapour to be changed into water, than an equal diminution in winter would do."—Glaisher *in Phil. Trans.* 1847.

"It is no uncommon thing in spring for the dew-point to be more than 20° below the temperature of the atmosphere in the shade, and I have even seen the difference amount to 30°. The effect of such a degree of dryness is parching in the extreme, and if accompanied with wind is destructive to the blossoms of tender plants."—Daniell's *Met. Essays.*

Howard, the meteorologist, found dews so abundant in autumn as to be capable of daily measurement in the rain-gauge.

[7] This fact may be readily illustrated by reference to Glaisher's *Hygrometrical Tables.* The atmospheric pressure being represented by a barometric column of 30 inches of mercury, air at the temperature of

		Grains		
50°	will be saturated by	4·1	of water.	Differences.
55°	„ „ „ „	4·9	„ „ „	0·8
60°	„ „ „ „	5·8	„ „ „	0·9
65°	„ „ „ „	6·8	„ „ „	1·0

Thus, while the temperature of the air rises by equal gradations, its capacity for moisture rises by increasing increments.

during the night, must bring it, if the temperature be high, much nearer to the point of repletion, before it be acted upon by the immediate cause of dew, than if the temperature were low.[8] We read, accordingly, in the writings of those who have travelled into hot climates, of a copiousness of dew frequently observed by them there, which very much exceeds what occurs at any time in this country. But even here dew, though for the most part scanty in our hottest season, is sometimes very abundant during it, an example of which occurred to me on the night common to the 29th and 30th of July, 1813; for on that night, notwithstanding its shortness, more dew appeared than has ever been observed by me on any other.

In the last place, I always found, when the clearness and stillness of the atmosphere were the same, that more dew was formed between midnight and sunrise than between sunset and midnight, though the positive quantity of moisture in the air must have been less in the former than in the latter time, in consequence of a previous precipitation of part of it. The reason, no doubt, is the cold of the atmosphere being greater in the latter than in the prior part of the night.

But there are many circumstances influencing the quantity of dew,[9] which, though much more open to accurate

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From the contents of note 7, it follows that, supposing air at 65°, and saturated with vapour, to fall to 60°, it would lose by condensation 1 grain of water; but, supposing the air at 55°, also saturated, to fall to 50°, it would lose only 0·8 grains. Therefore, viewing the deposition of dew with regard to temperature alone, it is evident that the higher the temperature the more humid the air will become for any given reduction of temperature.

Hygrometry, which in the time of Wells was in a very imperfect state, has since been greatly developed by the inventions of Daniell, Mason, Regnault, and others; while the Observatory at Greenwich has furnished observations which have led to the perfecting of the Tables. It is now an exceedingly useful and interesting branch of meteorology.

9 Various attempts have been made to determine the annual deposition of dew, but without success. To measure the quantity deposited under the most favourable circumstances is no easy task; and, although Mr. Daniell

observation than those hitherto mentioned, are yet much less easy to be understood.

In my first attempts to compare the quantities of dew formed during different times, or in different situations, I attended only to the appearance which it made on bodies having smooth surfaces. But quickly seeing this method to be very imperfect, I next employed wool to collect dew from the atmosphere, and found it well adapted for my purpose, as it readily admits amongst its fibres the moisture which forms on its outer parts, and retains what it receives so firmly, that I never but once had occasion to suspect that it suffered any portion of what it had thus acquired to pass entirely through it. The wool[10] which I used was white, moderately fine, and already imbued with a little moisture, from having been long exposed to the air of a room in which no fire was kept. I divided it into parcels of 10 grains each, and, immediately before exposure, pulled the fibres of every parcel somewhat asunder, so as to give it the form of a flattened sphere, the greatest diameter of which was about 2 inches. As in doing this I went by the judgment of my sight alone, some little inequality in point of size must have existed among different

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observes that the quantity of vapour existing in the atmosphere in the different seasons, at the level of the sea, seems to follow the course of the mean temperature, such are the varied and uncertain conditions connected with the deposition of dew, that no very accurate results for the average annual deposit can be obtained in the present state of our knowledge. It must, indeed, vary with every climate and every locality. In a general way it has been estimated for England at about five inches annually, which is computed to be about a seventh part of the average quantity of moisture received by the atmosphere in the same time.

[10] In any investigation upon dew it is important to employ a substance which will admit of its ready formation, and retain it for examination. Wool has been found very suitable, but care must be taken in selecting and in using it. Uniformity of fibre is necessary, and the parcels selected or made up for comparative observations should expose equal surfaces to the sky. Equality of mass or weight by no means implies equality of radiating surface.

parcels, but none, I think, sufficient to affect the accuracy of my conclusions from the experiments in which they were employed, more especially as my conclusions scarcely ever rested upon single trials.

Previously to mentioning the results of any of my experiments with these parcels of wool, I think it right to describe the place where by far the greater part of my observations on dew were made. This was a garden in Surrey, distant, by the public road, about three miles from the bridge over the Thames at Blackfriars, but not more than a mile and a quarter from a densely-built part of the suburbs on the south side of that river. The form of the garden was oblong, its extent nearly half an acre, and its surface level. At one end was a dwelling-house of moderate size, at the other a range of low buildings; on one side a row of high trees, on the other a low fence, dividing it from another garden. If this fence had been absent, the garden would have been on the latter side entirely open. Within it were some fruit-trees, but, as it had not been long made, their size was small. Towards one end there was a grassplat, in length 62 feet, and nearly 16 broad, the herbage of which was kept short by frequent mowing. The rest of the garden was employed for the production of culinary vegetables. All of these circumstances, however trifling they may appear, had influence on my experiments, and most of them, as will hereafter be seen, must have rendered the results less remarkable than they would have been if they had occurred on a wide open plain, considerably distant from a large city.[11]

[11] The example set by Wells, in giving this careful description of the place where he chiefly made his experiments, is well worthy of the attention of meteorologists. Numerous registers of temperature and rainfall observations have been found next to useless from the absence of these desirable details. They are more especially required in connection with observations

I now proceed to relate the influence which several differences in the situation, mechanical state, and real nature of bodies, have upon the production of dew:

I. One general fact relative to situation is, that whatever diminishes the view of the sky, as seen from the exposed body, occasions the quantity of dew which is formed upon it to be less than would have occurred if the exposure to the sky had been complete.

I placed, on several clear and still nights, 10 grains of wool upon the middle of a painted board, 4½ feet long, 2 feet wide, and 1 inch thick, elevated 4 feet above the grassplat, by means of four slender wooden props of equal height; and, at the same time, attached loosely, 10 grains of wool to the middle of its underside. The two parcels were consequently only an inch asunder, and were equally exposed to the action of the air. Upon one night, however, I found that the upper parcel had gained 14 grains in weight, but the lower only 4. On the second night the quantities of moisture, acquired by like parcels of wool, in the same situations as in the first experiment, were 19 and 6 grains; on a third, 11 and 2; on a fourth, 20 and 4; the smaller quantity being always that which was gained by the wool attached to the lower side of the board.

I bent a sheet of pasteboard into the shape of a house-roof, making the angle of flexure 90 degrees, and leaving both ends open. This was placed one evening, with its ridge uppermost, upon the same grassplat, in the direction of the wind as well as this could be ascertained. I then laid 10 grains of wool on the middle of that part of the grass which was sheltered by the roof, and the same quantity on another

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upon solar and terrestrial radiation, the rainfall and evaporation, the registration of ozone, the maximum and minimum temperature in the shade. The introduction of such particulars regarding local position and circumstances should be deemed an essential duty by all who keep a register of any such meteorological observations.

part of the grassplat fully exposed to the sky. In the morning the sheltered wool was found to have increased in weight only 2 grains, but that which had been exposed to the sky, 16 grains.

In these experiments the view of the sky was almost entirely cut off from the situations in which little dew was formed. In others, where it was less so, the quantity gained was greater. Thus, 10 grains of wool, placed upon the spot of the grassplat which was directly under the middle of the raised board, and which enjoyed, therefore, a considerable oblique view of the sky, acquired during one night 7, during a second 9, and during a third 12 grains of moisture, while the quantities gained during the same times, by equal parcels of wool laid upon another part of the grassplat, which was entirely exposed to the heavens, were 10, 16, and 20 grains.

As no moisture, falling like rain from the atmosphere, could, on a calm night, have reached the wool in any of the situations where little dew was formed, it may be thought that the substances under which the wool was placed prevented, mechanically, the access of that fluid. But on this supposition it cannot be explained why some dew was always found in the most sheltered places, and why a considerable quantity occurred upon the grass under the middle of the raised board. A still stronger proof of the want of justness in this supposition is afforded by the following experiment: —I placed, upright, on the grassplat a hollow cylinder of baked clay, the height of which was 2½ feet, and diameter 1 foot. On the grass, surrounded by the cylinder, were laid 10 grains of wool, which, in this situation, as there was not the least wind, would have received as much rain as a like quantity of wool fully exposed to the sky. But the quantity of moisture obtained by the wool surrounded by the cylinder was only a little more than 2 grains, while that acquired by 10 grains of fully exposed wool was 16. This occurred on the night, during which the wool under the bent pasteboard gained only 2 grains of moisture.[12]

Dew, however, will, in consequence of other varieties of situation, form in very different quantities, upon substances of the same kind, although these should be similarly exposed to the sky.

In the first place, it is requisite for the most abundant formation of dew, that the substance attracting it should rest on a stable horizontal body of some extent. Thus, upon one night, while 10 grains of wool, laid upon the raised board, increased 20 grains in weight, an equal quantity, suspended in the open air, 5½ feet above the ground, increased only 11 grains, notwithstanding that it presented a greater surface to the air than the other parcel. On another night, 10 grains of wool gained on the raised board 19 grains, but the same quantity suspended in the air, on a level with the board, only 13; and on a third, 10 grains of wool acquired, on the same board, 2½ grains of weight, during the time in which other 10 grains, hung in the air, at the same height, acquired only ½ a grain.

In the second place, the quantities of dew attracted by equal masses of wool, similarly exposed to the sky, and resting on equally stable and extended bodies, oftentimes vary considerably, in consequence of some difference in the other circumstances of these bodies. Ten grains of wool, for instance, having been placed upon the grassplat on a dewy evening, 10 grains upon a gravel walk which bounded the

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[12] "Harvey traced the effect of a diminished aspect of the sky by employing cylindrical, and the frustums of conical, vessels of a common diameter at the base and of a common altitude. The greater the upper diameter of a vessel, all other things being the same, the greater was the amount of dew on equal masses of wool placed at the bottom; and a sensible diminution in the quantity of dew was perceived, whenever the upper end of a vessel had only a few narrow detached slips of wood placed above it."—*Enc. Met.*, p. 134, vol. v.

grassplat, and 10 grains upon a bed of bare garden mould, immediately adjoining the gravel walk; in the morning, the wool on the grass was found to have increased 16 grains in weight, but that on the gravel walk only 9, and that on the garden mould only 8. On another night, during the time that 10 grains of wool, laid upon grass, acquired 2½ grains of moisture, the same quantity gained only ½ a grain upon the bed of garden mould, and a like quantity, placed upon the gravel walk, received no accession of weight whatever.

Two objections will probably be made against the accuracy of these as well as my other experiments with wool. One is, that wool placed on grass may, by a kind of capillary attraction, receive dew previously formed on the grass, in addition to its own. To this I answer, that wool in a china saucer, placed on the grass, acquired very nearly as much weight as an equal parcel immediately touching the grass. The second objection is, that a part of the increased weight in the wool might arise from its imbibing moisture as a hygroscopic substance. I do not deny that some weight was given to the wool in this way; but it may be safely affirmed that this quantity must have been very small. For, on very cloudy nights, apparently the best fitted to increase the weight of hygroscopic substances, wool upon the raised board would, in the course of many hours, acquire little or no weight; and in London, I have never found 10 grains of wool, exposed to the air on the outside of one of my chamber windows, to increase during a whole night more than half a grain in weight. When this weight was gained the weather was clear and still; if the weather was cloudy and windy, the wool received either less or no weight. This window is so situated as to be, in great measure, deprived of the aspect of the sky.

It being shown that wool, though highly attractive of dew, was prevented by the mere vicinity of a gravel walk, or a bed of garden mould, for only a small part of it actually touched those bodies, from acquiring nearly as much dew as an equal parcel laid upon grass, it may be readily inferred that little was formed upon themselves. In confirmation of this conclusion I shall mention that I never saw dew upon either of them. Another fact of the same kind is, that while returning to London from the scene of my experiments about sunrise, I never observed if the atmosphere was clear, the public road or any stone pavement on the side of it to be moistened with dew, though grass within a few feet of it and painted doors and windows of houses not far from it were frequently very wet. If, indeed, there was a foggy morning, after a clear and calm night, even the streets of London would sometimes be moist though they had been dry the day before, and no rain had in the meanwhile fallen. This entire, or almost entire, freedom of certain situations from dew depends, however, much more upon extraneous circumstances than upon the nature of the substances found there; for river sand, though of the same nature with gravel, when placed upon the raised board, or upon grass, attracted dew copiously.

A third difference from situation in the quantity of dew collected by similar bodies similarly exposed to the sky depends upon their position with respect to the ground. Thus, a substance placed several feet above the ground, though in this situation later dewed than if it touched the earth, would, notwithstanding, if it lay upon a stable body of some extent, such as the raised board lately mentioned, acquire more dew during a very still night than a similar substance lying on grass.

A fourth difference of this kind occurred among bodies placed on different parts of the raised board. For one that was placed at the leeward end of it, generally acquired more dew than a similar body at the windward extremity.

II. Difference in the mechanical state of bodies, though all other circumstances be similar, has likewise an effect on the quantity of dew which they attract. Thus more dew is formed upon fine shavings of wood than upon a thick piece of the same substance. It is chiefly for a similar reason, I believe, that fine raw silk, fine unwrought cotton, and flax were found by me to attract somewhat more dew than the wool I employed, the fibres of which were thicker than those of the other substances just mentioned.

III. Bright metals, in consequence of some circumstance in their constitution, attract dew much less powerfully than other bodies; all of which, after allowance has been made for any difference which may exist in their mechanical states, seem to attract dew in quantities not very unequal if they be similarly situated.

Musschenbroek was the first who distinctly remarked this peculiarity of metals, but Dufay,* I believe, published it before him, referring, at the same time, the discovery to its proper author. Both Musschenbroek and Dufay, however, made too large an inference from their experiments, for they asserted that dew never appears on the upper surface of bright metals, whereas the contrary has since been observed by many persons, and I have myself known dew to form on gold, silver, copper, tin, platina, iron, steel, zinc, and lead. Dew, however, when it does form upon metals, commonly sullies only the lustre of their surface; and even when it is sufficiently abundant to gather into drops, these are almost always small and distinct. Two other facts of the same kind are—*first,* that the dew which has formed upon a metal will often disappear, while other substances in their neighbourhood remain wet; and *secondly,* that a metal which has been purposely moistened will often become dry, though similarly exposed with bodies which are attracting dew. This inaptitude

* Mem. de l'Acad. Fran. 1736.

to attract dew in metals is communicated to bodies of a very different nature which touch, or are near to them. For I have found that wool laid upon a metal will acquire much less dew than an equal quantity laid upon grass in the immediate vicinity.

A large metallic plate, lying on grass, resists the formation of dew more powerfully than a very small one similarly situated. I conclude from various collateral facts that a considerable difference in the thickness of two pieces of metal exposing equal surfaces to the sky will be attended with a similar consequence wherever they be placed, though I have no observation which proves this directly. If, however, a large and a very small plate be suspended horizontally at the same height in the air the small plate will resist the formation of dew more powerfully than the large.

If a metal be closely attached to a substance of some thickness which attracts dew powerfully, the attraction of the metal itself for dew, instead of being increased from this circumstance, becomes diminished, provided the metal cover the whole of the upper surface of the other body. If only a part of this body be covered the production of dew on the metal is forwarded by the conjunction, and this somewhat in proportion to the quantity of surface in the lower body left uncovered. The justness of the first of these observations is proved by the following experiment. I joined, in the form of a cross, two pieces of very light wood, each 4 inches long, a third of an inch in breadth, and 1 line in thickness. To one side of this cross I fastened, by means of mucilage, a square piece of gilt paper, and then exposed the instrument to the sky with its metallic side uppermost on a dewy night, by suspending it in a horizontal position, about 6 inches above the ground. A few hours after the unattached parts of the metalled paper were found covered with minute drops of dew, while those which adhered to the cross were dry.

A large metallic plate laid upon grass was dewed with more difficulty on its upper surface than a similar plate elevated a few inches above the grass by means of slender props, which allowed the air to pass freely under the metal. But the case with respect to small pieces was the reverse; for I have often seen, covered with dew, the metallic sheath of a small thermometer lying upon grass, while the similar sheath of another thermometer, suspended in the air, remained dry.

Removing a metal several times in the course of the night from one part of the grassplat to another, facilitated its being dewed. The same effect was produced on gilt and silvered paper, by first exposing them to the sky for some time, with the bare side uppermost, and then turning them.

If a piece of glass, covered on one side with a metal, be placed upon the ground with this side downwards, the upper surface will attract dew, precisely as if no metal were attached to the lower surface.

The upper surfaces of metals are most readily and most copiously dewed on those nights and in those parts of the night, during which other substances are the most readily and the most copiously dewed.

If a metallic plate had been laid upon grass, before dew began to form anywhere, its lower side, notwithstanding, always became moist in the course of the night; and the same effect was almost always observed, if the plate had been placed horizontally in the air, a few inches above the grass. While the undersides were thus moist the upper surfaces were very often dry. If, however, the plate was elevated several feet in the air, the condition of both sides was always the same, whether this was dry or moist.

The remarks hitherto made on the relation of metals to dew, apply to the class generally; but it is now to be mentioned that they do not all resist the formation of that fluid with the same force.

I saw, for example, platina one night distinctly dewed, while gold, silver, copper, and tin, though similarly situated, were entirely dry; and I have also several times seen these four metals free from dew, while iron, steel, zinc, and lead were covered with it.

I once supposed, in consequence of the difficulty with which metals are dewed, that they might in all circumstances resist, in a greater degree than other bodies, the condensation of watery vapour upon their surface; and I afterwards found that Le Roi* asserts this to be the case. But having exposed at the same time, to the steam of warm water, pieces of glass and of metal, I did not see that moisture formed in the least more readily upon the former than upon the latter. I have since learned that Saussure† once entertained a similar suspicion, which was also proved by an experiment to be groundless.

All my experiments hitherto spoken of were made in the country. But Le Roi having said that dew is never deposited by the air of cities, I determined to ascertain if his assertion was just. With this view I frequently exposed at night 10 grains of wool upon a slight wooden frame, placed in such a manner between two ridges of the top of my house, which is situated in one of the most crowded districts of London, as to be 3 feet distant from the nearest part of the roof. The event was, that upon clear and calm nights dew was always acquired by the wool, though never in any considerable quantity; probably, however, more from the wooden frame being nearly surrounded by buildings much more elevated than itself, than from any particular condition of the

* Mem. de l'Acad. Fran. 1751. † Hygronometrie, page 329.

air in cities. The formation of dew in this situation proceeded much less regularly than in the country. For, upon one evening 10 grains of wool gained in it 3 grains of moisture in 1 hour and 18 minutes, though I scarcely ever knew a greater quantity to be collected by a similar parcel of wool in the same place during a whole night. These experiments will no doubt seem to many superfluous, since dew may be observed every fine evening upon grass in London. But as dew upon grass is said by Le Roi to proceed from the ground and not from the atmosphere, the argument derived from its appearance there, in cities, against his assertion is thus eluded by him.

The last subject which I shall here touch upon, is that of hoarfrost.

This substance has, I believe, from the time of Aristotle,* been uniformly, and, according to my observations, justly considered as frozen dew. I shall, therefore, frequently refer hereafter to the experiments of the late Mr. Patrick Wilson, of Glasgow, respecting it, as if they had been actually made upon that fluid. Indeed several of my experiments upon dew were only imitations of some which had been previously made upon hoarfrost by that ingenious and most worthy man.

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* Meteor. Lib. I. c. x.

SECTION II.

Of the Cold connected with the Formation of Dew.[13]

Dew is often spoken of as being cold, by popular writers. Thus Cicero and Virgil apply to it the epithet of "gelidus," Milton that of "chill," and Collins that of "cold." Of the same import is a passage in Herodotus, in which it is said, that in Egypt the crocodile passes a great part of the day on dry land, but the whole of the night in the Nile, this being warmer than the atmosphere and the *dew*. Among philosophers, however, Mr. Wilson was the first, I believe, who ever suspected the existence of such a conjunction.[14]

In my experiments on the temperature of bodies moistened with dew, small thermometers were employed (the largest being only 8 inches long) having globular bulbs, which in most of them were not more than from 2 to 2½ lines in diameter. Their scales, which were marked in the manner

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[13] All the writers and experimentalists on dew, preceding Wells, considered the cold as the *effect* of the dew instead of its being the *cause*. Patrick Wilson, however, appears to have been close upon a true conviction on the subject, for in giving the account of his experiments in 1780 he said, it would be going too far to conclude "*that very cold air is never disposed to deposit its contents except upon bodies as cold, or colder than itself; and yet that this is frequently the case seems probable from a number of common appearances.*"

[14] That Wilson was the first philosopher who suspected that dew was colder than the atmosphere may be questioned. The Hon. Robert Boyle's essay entitled "*Experimental History of Cold,*" published in 1665, contains evidence that he had experimentally shown that *dew* and *hoarfrost* are formed by the precipitation of aqueous vapour contained in the air upon a colder body; he, however, appears never to have realised the resemblance between the production of dew artificially upon a cold body, and its natural formation upon the cold surface of the earth at night.

of Fahrenheit, were of ivory or wood, and were furnished, almost all of them, with hinges. They were always employed naked, except I wished to know the effect of covering them with any particular substance.[15]

By means of these instruments I have very many times, during serene and still nights, examined the temperature of dewed grass, and have constantly observed it to be less than that of the air, anywhere between 1 inch and 9 feet above the ground, the latter being the greatest height at which I ever marked the heat of the atmosphere in these experiments. I generally, however, compared the temperature of dewed grass with that of the air 4 feet above the ground; and on nights that were calm and clear very frequently found the grass at the ordinary place of my observations 7, 8, or 9 degrees colder than the air at that height. Several times it was 10 and 11 colder than the air, and once 12°. These differences are not so great as those related in Mr. Six's posthumous work. But in his experiments the temperature of grass was compared with that of the air 7 feet above the ground, which, in clear and calm nights may be regarded as half a degree warmer than the air at the height of 4 feet. Besides, the most considerable differences mentioned by Mr. Six, occurred in winter, when he says a greater degree of cold is occasioned by dew than at any other time, whereas very few of my experiments on the temperature of grass were instituted in that season. In the last place my experiments

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[15] During the last fifty years improvements have been effected in the construction of thermometers, as in other things. Such instruments as Wells employed would now be deemed ill-adapted for investigations upon nocturnal chilling and the phenomena of dew. It would appear that they were graduated only on their frames. The improvements of engraving the scale on the stem itself and of enamelling white the tube behind the scale, have rendered the thermometer an instrument of the utmost precision if skilfully made. J. Glaisher, F.R.S., was the first to employ really good thermometers for the purpose of prosecuting experiments similar to those of Wells.

were almost always made on very short grass, while Mr. Six's thermometers were laid upon long grass bent by strong pressure towards the earth, in which state they marked a temperature 1, 2, and 3 degrees lower than that shown by similar thermometers placed upon grass less than an inch in height.[16] Had it not been for these circumstances and the unfitness in various respects, besides the shortness of the grass, for the production of a great cold, of the common scene of my operations, I believe that in consequence of my thermometers being much better adapted to mark a superficial or transitory cold, than those of Mr. Six, I should at some time have seen a difference several degrees greater than the greatest ever seen by that gentleman, which was one of 13°½. In confirmation of this opinion I shall mention, that having, during a short visit to a more distant part of the country, exposed in the evening a thermometer upon the surface of an open grass field, I found it soon after, although the grass was short and the weather warm, 14° lower than a similar thermometer suspended in the air, 4 feet above the grass. If to this quantity be added ½ a degree, on account of the difference in elevation between our suspended thermo-

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[16] James Six, F.R.S., writing in 1783, said:—"On green turf where the grass is not an inch long, the diminution [of temperature] will be less on some nights by 1°, 2°, or 3°, than where the grass is longer, and accordingly as the cold is more or less intense."

The radiating power of grass appears from Mr. Glaisher's experiments to be such that a thermometer when placed on long grass reads less than when placed on short grass by 1°·1; that the temperature of the soil under long grass exceeds that under short grass by 1°·1; being exactly the same amount of excess under as it was in defect on the top. "Hence the cause of the difference of the readings on the top of long and short grass arises solely from the greater quantity of heat conducted to the surface of the latter from the soil, over that conducted to the surface of the former, and not from the greater quantity of heat radiated into space from the long, over that radiated from the short; such being the case, it was to be expected that the readings of a thermometer would vary with every variation of the length of grass upon which it was placed, and such was found to be the case."

meters, the cold connected with dew observed by me this night on grass will exceed the greatest ever observed by Mr. Six by 1 degree.

According to a few observations made by me, the greater coldness of grass than that of the air, begins to appear in clear and calm weather, in places sheltered in the afternoon from the sun, but still open to a considerable portion of the sky, soon after the heat of the atmosphere has declined. A similar coldness continues upon grass in still and serene mornings for some time after the rising of the sun, in places shaded from its direct light but otherwise open to the sky. My experiments on this point have also not been many, and none of them were made in winter; which, I presume, are the reasons that I never observed a cold from this cause later in the morning than an hour after sunrise. The surface of snow, however, was once, in the depth of winter, observed by Mr. Wilson,[17]

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[17] In January, 1768, Professor Alexander Wilson noticed that a thermometer placed on snow marked a temperature 8° lower than when suspended in the air, and that the mercury always rose a trifle when a mistiness came on, and *vice versa*. (Paper in Phil. Trans., 1771.) His son, Professor Patrick Wilson, afterwards took up the investigation of this phenomenon; and to him must be assigned the credit of having been the first to recognise the important fact that the ground at night, during clear weather, is colder than the superincumbent air. Tomlinson says that the following interesting Table, made by P. Wilson, was the first of the kind ever published :—

Thursday Evening, January 13th, 1780.

Time.			Therm. on Snow.	Therm. in Air.
8h.	30m.		— 12°	0°
9			— 14	— 2
10			— 14	— 4
11			— 17	— 6
11	30		— 18	— 6
12	30	Friday	— 20	— 8
1		Morning	— 23	— 7
1	30		— 22	— 8
2			— 22	— 9
2	30		— 21	— 8
3			......	— 9
3	30		......	— 10
4			......	— 12
4	30		......	— 12
5			......	— 12

of Glasgow, to be considerably colder than the air, till a little after midday.* [18]

In cloudy nights, particularly if there was wind, the grass was never much colder than the air. On such nights the temperatures of both were sometimes the same; at other times that of the grass was the higher of the two, even when the grass was wet from preceding rain, and when consequently it must have been in some measure cooled by evaporation. On one such night the grass was found to be 4° colder than the earth an inch beneath the surface of the plat, which afforded a sufficient reason for the grass itself being warmer than the air. In windy weather, however, if the sky was clear some degree of cold in addition to that of the air was always observed upon the grass; and in calm weather very high

5 30	......	— 12
6	......	— 14
6 30	— 22	— 13
7	— 22	— 13
7 30	— 22	— 13
8	— 19	— 10

"Wilson soon found that the cause of these remarkable differences could not be attributed to the evaporation of the snow, seeing that hoar-frost was being abundantly deposited at the time when the readings were lowest. On the night of the 23rd January 'several things were laid out at the Observatory, such as sheets of brown paper, pieces of boards, plates of metal, glasses of several kinds, &c., which all began to attract hoar-frost, seemingly as soon as they had time to cool down *to* the temperature of the air. The sheets of brown paper, being thin and easily cooled, acquired it soonest, and when beheld by candlelight were beautifully spangled over by innumerable reflections from the minute crystals of hoar-frost which had parted from the air.'

"If, in the above passage, the writer, instead of the little word *to,* which I have printed in italics, could have written the word *below,* he would have given the true theory of the formation of dew and hoar-frost."—*Tomlinson.*

* Paper in Phil. Trans. 1781.

[18] The observations were taken on 25th January, 1781, and are as follows:—

Time.		Thermometer in Air.	Thermometer on Snow.
9h. 45m. A.M.		10°	3°
10 30		14	4
11 30		17	9
12 30 P.M.	cloudy	22	20
1	more cloudy	25	26
1 30	cloudy all over	27	27

clouds, though sufficiently extensive and dense to conceal the sky completely, would yet frequently allow of the grass being several degrees colder than the air.[19] I once observed upon a night of this kind a difference of 5° between the temperatures of those bodies.

If the night became cloudy after having been very clear, though there might be no change with respect to calmness, a considerable alteration in the temperature of the grass always ensued; and this sometimes very suddenly. Upon one such night the grass, after having been 12° colder than the air, became only 2° colder than it, the temperature of the air being the same at both observations. On the second night, grass became 9° warmer in the space of an hour and a half. On a third night, in less than forty-five minutes—for the whole change occurred while I was absent forty-five minutes, the temperature of the grass rose 15°, while that of the neighbouring air increased 3°½. During a fourth night the temperature of the grass at half-past 9 o'clock, was 32°. In twenty minutes afterwards it was found to be 39°, the sky having in the mean time become cloudy. At the end of twenty minutes more, the sky being clear, the temperature of the grass was again 32°. These were the most remarkable of my observations on this subject; but I may add to them, that I have frequently seen, during nights that were generally clear, a thermometer lying on the grass-plat rise several degrees, upon the zenith being occupied only a few minutes by a cloud. On the other hand, upon two nights I observed a very great degree of cold to occur on the ground, in addition to that of the atmosphere, during

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[19] By virtue of the law of the reciprocal radiation of bodies, "the clouds return to the earth the whole, or a great part, of the heat which may have been radiated from it; although dense clouds may sometimes be formed so high as to be colder than the earth, and thus to radiate to it less heat than the ground imparts; accounting, therefore, for the phenomenon sometimes observed on cloudy nights of good radiating bodies being several degrees colder than the air."—*Harvey*.

short intervals of clearness of sky, between very cloudy states of it.[20]

I did not speak in the preceding section of another obscure state of the atmosphere—that occasioned by fog, or mist—as the moisture deposited in it attaches to all bodies indiscriminately; on which account I was unable to determine whether or not dew forms during its continuance. But, with respect to the connexion of this condition of the atmosphere with cold, I have to remark that I have several times, on its appearance betwixt daybreak and sunrise, found the difference between thermometers on grass and in the air, which had been considerable during the night, to diminish greatly. I never, indeed, observed it to vanish; but this I used to impute to the air being not very much obscured. I have now, however, reason to doubt the justness of this conclusion; for on the evening of the 1st of January in the present year, 1814, I found, during a dense fog, while the weather was very calm, a thermometer lying on grass, thickly covered with hoarfrost, 9° lower than another suspended in the air, four feet above the former. On the following evening, when the air was equally calm, but the fog sufficiently attenuated to allow me to see that the sky was almost entirely covered with clouds, the difference between two thermometers, similarly placed with the former, was only 1°. On

[20] By tabulation of numerous observations, Mr. Glashier says, "it appears that at times when the sky was entirely covered with low cirrostratus clouds, the readings of a thermometer placed on long grass was the same as that in the air; that, with the same clouds at a moderate elevation, the reading of the thermometer in air has exceeded that on long grass by 3°; and on those clouds being high, this excess has amounted frequently to 5°; and if other clouds than cirrostratus covered the whole sky, this excess has been as large as 10°. At times when the sky has been free from clouds, but not bright, haze and vapour being prevalent, the above excess has amounted to 10°, 11, or 12°; and at times when the sky has been both bright and clear, with the air calm, no mist, haze, vapour, or fog being prevalent, this difference has frequently amounted to 14°, less frequently to 19°, and sometimes to 20°."

comparing the observations of these two evenings, I conclude that, on the first, few or no clouds existed above the fog, and consequently that fog, if there be no clouds above it, may, in a very calm air, admit of the appearance of a considerable degree of cold at night upon the surface of the earth, in addition to that of the atmosphere. Mr. Six, indeed, says, while speaking of the cold connected with dew, in his paper in the Philosophical Transactions for 1788, "fogs did not, as far as I could perceive, at all impede, but rather increase, the refrigeration." But this was a mistake, which in all probability arose from his ascribing the effect of a clear night to an ensuing foggy morning, as he examined his thermometers only in the daytime. He afterwards discovered his error; for, in his posthumous work, thick fogs are ranked among the circumstances which always impede, and sometimes prevent altogether, the appearance of a cold upon the surface of the earth, greater than that of the atmosphere. During a very dense fog, Mr. Wilson found no difference at night between a thermometer laid upon snow and another suspended in the air.*

When, during a clear and still night, different thermometers were examined at the same time, which had been placed in different situations, those which were situated where most dew was formed were always found to be the lowest.[21] Thus, upon one such night I found a thermometer placed upon a little wool, lying upon the middle of the upper side of the raised board, to be 9° lower than another thermometer in contact with an equal quantity of wool attached to the middle of the underside of the board. On two other nights the difference between two thermometers in the same situations was 8°. I found also, on two other serene and calm nights, a spot of grass covered by the pasteboard roof,

* Edin. Phil. Trans. I. 170.

[21] "Starlight nights, yea, and bright moonshine nights, are colder than cloudy nights."—*Lord Bacon.*

and another spot surrounded by the earthen cylinder, to be both 10° warmer than neighbouring grass fully exposed to the sky. Thinking it possible that the cylinder, which had been exposed to the sun the preceding day, might still possess some of the heat which it had then imbibed, I placed near to it, on another night, a cylinder made of very thin pasteboard; but this was equally efficacious with the earthen one in preventing cold from occurring on grass. When the exposure was greater than in the preceding examples, and more dew was in consequence formed, the cold was also greater, but still less than where the exposure was complete.[22] For instance, upon the night during which 10 grains of wool, placed upon the middle of the grass, which was sheltered by the raised board, had gained 7 grains, and the same quantity on grass fully exposed to the sky had gained 10 grains, the difference between the temperatures of the two portions of grass was only 2°½.

The same correspondence was observed when the differences in the quantity of dew did not depend, as in the

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[22] "I placed a large sheet of pasteboard vertically on the grassplat, and laid a thermometer close to its lower edge on each side of it; the readings of both these thermometers thus placed were found to be identical and intermediate to that placed in air at the height of four feet, and protected from the effects of radiation, and that placed on grass fully exposed to the sky; the same relation was found to exist in what azimuth soever the board was placed.

"I then placed the pasteboard at an angle of 45° nearly with the horizon, and laid the thermometers as before. In this situation the readings were found to be intermediate as before; but that which was exposed to three-fourths of the sky read most nearly to that on the grass fully exposed to the sky, and the one which was exposed to one-fourth part of the sky only, read most nearly to that in air, The amount of these differences of readings was as nearly as could be determined exactly proportional to the amount of the exposed sky.

"Hence, as a general fact it may be considered, that whatever diminishes the view of the sky as seen from an exposed body, causes its temperature to decrease less than it would if the exposure to the sky be complete."—Glashier, *Phil. Trans.* 1847.

preceding instances, upon any diversity of exposure to the sky. Thus, the mercury in a thermometer placed upon wool, lying on the raised board, was found to be at the 44th degree, while that in another, pendent in the air, at the same height from the ground, and wrapped in wool, was at the 48th. Wool also, on the raised board,* was commonly a little colder than the same substance on grass, when the night was very still; and the leeward end of that board was generally colder than the windward extremity.

But the most remarkable examples of this kind were exhibited by the gravel walk and the bare garden mould. In still and serene nights the surfaces of these bodies were always warmer than the neighbouring grass, and frequently warmer than the air. On one night of this description I observed, two hours and a half after sunset, the surface of the gravel walk to be 16°½, and that of the garden mould to be 12°½ warmer than grass very near to them; and similarly exposed to the heavens. As the night proceeded clouds formed and accumulated; in consequence of which the difference at sunrise, between the temperature of the grass and the gravel walk was only 6°, and between those of the grass and the mould only 4°, the temperature of the grass having in the mean time increased considerably, while that of the other bodies had decreased a little. At another time, shortly before sunrise, a very clear morning having succeeded a cloudy night, I found the gravel walk to be 10° and the garden bed to be 9° warmer than neighbouring grass, which

* The greater cold of the raised board, in my experiments, most probably depended on the grass being very short; since Mr. Wilson found that snow on the ground was colder than the same body on a raised board. If one, two, or three degrees were added to the cold of the grass at my place of observation, agreeably to the difference found by Mr. Six, between the temperatures of long and short grass in dewy nights, the cold on my raised board would, upon such nights, have been always less than that of the grass-plat.

was 8° colder than the air. Both of these examples occurred in summer, and I believe that such considerable differences will occur in that season only. It was on the first of these two nights that 10 grains of wool gained only half a grain of moisture on the mould, and that the same quantity gained no weight on the gravel walk. That the unfitness of the gravel walk, however, to become cold, like its unfitness to attract dew, arose from its situation, and not from the nature of the substance of which it was made, is proved by this circumstance that river sand, placed on the raised board, was on four different nights, none of them highly favourable for the production of cold, 7, 7, 8 and 8½ degrees colder than the air at the same height.

It may be added here, that I have always found, on dewy nights, the temperature of the earth, half an inch or an inch beneath its surface, much warmer than the grass upon it. On five such nights the differences were from 12 to 16 degrees. The earth, at the above-mentioned depth, was also almost constantly warmer on dewy nights than the air; sometimes it was considerably so, for I once observed it to be 10° warmer, at another time 9°, and at a third 7°½. An exception will no doubt occur, if very mild weather should follow a long frost; but of this I have had no experience.

In the experiments upon my housetop in London, I always found, during clear and calm nights, wool lying on the wooden frame to be colder than the air at the same height; but the difference was seldom more than 3°. On the evening, however, during which dew formed there more copiously than usual, the difference was 5°. That the smallness of these differences was not wholly occasioned by anything special in the air of cities was afterwards proved by my finding others much greater, in a garden nearly in the middle of London, from which almost the whole of the sky was visible.

Metals, likewise, furnish proofs of the connexion of dew with a cold in the substance on which it forms, superior to that of the neighbouring atmosphere. My observations, however, on the temperature of metals when exposed to the sky on dewy nights, were less numerous than those on several other subjects treated in this Essay, by reason of the less frequent opportunity I enjoyed of making them; and many of those which I did make were afterwards found by me to have been improperly conducted. I thought, for instance, for some time that the temperature of a metal on a dewy night might easily be learned in the way in which I had been accustomed to ascertain the temperature of dewed grass. But, observing dew one night on the glass tube of a thermometer, which was lying on a metal placed upon grass, while the metal itself was free from moisture, I conceived it probable that the cold then indicated by the thermometer was not the real temperature of the body to which it was applied. To determine the point, I placed on the same metal a second thermometer, covered with gilt paper, upon which this was found at three observations to be 6°½, 7°, and 7° higher than the other.* In this experiment, the bulb of the naked thermometer, from being very small, did not project as far as the outer surface of the scale, and, consequently, did not come in contact with the metal. But, even when the ball of a thermometer was applied directly to a metal on a clear and calm night, a temperature was marked by it, commonly 2 and 3, and sometimes more degrees less than that marked by a similar thermometer enclosed in gilt paper, and similarly placed. I found it likewise necessary, in this enquiry, to correct the temperature of the air, as given by a naked thermometer. For, on still and serene nights, a thermometer inclosed in a case of gilt or silvered paper, and suspended in the air 4 feet above the grassplat,

* Page 42.

was usually observed to be 1°½ or 2° higher than a bare thermometer of the same construction suspended near to it. The difference of two such thermometers thus placed was once observed by me to be 2°½, and once 3°½.[23] It may be thought, perhaps, that these differences were caused by the metalled case obstructing the transmission of the temperature of the air to the enclosed instrument. But that this was not the reason is shown by my observing that on cloudy nights there existed no difference between the two thermometers; that, even on clear nights, a thermometer contained in a case of white paper, somewhat thicker than the metalled, was always nearly of the same temperature with a naked one which was suspended close to it; and that, when a difference did exist

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[23] "The formation of dew was found to depend solely on the temperature of the bodies upon which it was deposited, and it never appeared upon them till their temperatures had descended below that of the dew-point in their locality, as found by observations of a dry and wet-bulb thermometer placed in their vicinity.

"The amount of water thus deposited was the greatest upon the substances whose temperatures were the lowest; among these bodies glass was found to radiate heat freely, and it very readily became wet with dew. In consequence of this property the tube of a naked thermometer, which was lying on a substance entirely free from moisture, was frequently found covered by dew, and therefore it seemed probable that the temperature exhibited by the instrument was not that of the body in question. On such instances occurring an attempt was made to correct the error by enclosing the thermometer stem in a tube made of gilt paper; the bulb alone, resting on the substance, remained exposed to the sky. The differences between the readings of a thermometer thus enclosed, and when naked, were found to be sensible, but small in amount. It was observed that when the thermometer was wholly naked, the stem was at times wet when the bulb itself was dry; and at all times much less moisture appeared on the bulb than on the stem, unless the disposition of the substance in question to become cold was the same [as] or greater than, that of glass. The error arising from this cause was chiefly confined to the consequent contraction of the mercury in the stem, and not in the bulb, and which was considered to be avoided by the use of gilt paper: the error in all cases must have been small."—Glaisher, *Phil. Trans.*, 1847.

between the two latter, the thermometer in the white paper case was commonly lower than the other.[24]

The estimation of the heat, both of air and of metals, on a dewy night, is liable to errors from other causes. As these, however, are trifling, I shall not mention them, but proceed to state the results of my observations upon the temperature of metals exposed to the sky at night, though unable to vouch for their entire accuracy.

Thin bright metallic plates, the least having a surface of 25 square inches, and some of them a surface of more than 100 such inches, were several times observed, while lying on grass which was attracting dew, to be 1 and 2, and once 3 degrees warmer than the air 4 feet above them. At other times their temperature was the same with that of the air. In both of these cases their upper surfaces were always free from dew. Metals thus situated were, consequently, often much warmer than the grass which surrounded them. I made no experiments on this point during the nights on which occurred the greatest instances of cold on grass, relatively to the temperature of the air; but I found, notwithstanding, during one night, a metal on grass to be 10 warmer than the exposed grass near to it. On two other nights the differences were 9° and 8°. The superiority of the heat of metals on grass over that of the air, when it did exist, was evidently connected with the temperature of the grass which they covered,

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[24] "A glass thermometer, suspended in the air, will not give the temperature of the air; its own power as a radiant or an absorbent comes into play. On a clear day, when the sun shines, the thermometer will be warmer than the air; on a clear night, on the contrary, the thermometer will be colder than the air."—*Tyndall.*

"There is no doubt that if a thermometer be freely suspended in the air with its bulb at the height of thirteen feet above the soil, and far from any object to reflect heat to it, its readings will represent the true temperature of the air at the time, and much more truly than those of any one placed near the ground, or within a few feet of walls or buildings."—Glaisher, *Phil. Trans.*, 1847.

and this again with that of the earth under the same portion of grass; for this portion was always a little warmer than the metal, but not so warm as the earth.

On the other hand, metals on which dew was forming while they lay upon grass were always colder than the air. In like manner, if one metal upon the grassplat were dewed, while another similarly situated remained dry, the former was always colder than the latter.

When a metal lying on the grassplat became dewed, the grass under it was always colder than that under another metal which was undewed.

A metal, while receiving dew, in consequence of being elevated in the air, was always colder than a similar metal which remained undewed on the grass.

The greatest instances of cold observed by me on metals occurred at times when other bodies near to them had become considerably colder than the atmosphere.

The cold, however, contracted by metals, from exposure to the sky in a clear and still night, was always less than that of other bodies similarly situated, the greatest excess of cold ever observed by me in the larger metallic plates from this cause over that of the air being not more than 3 or 4 degrees. If much smaller pieces were placed upon grass, the result was different. For I have found a small thermometer placed in this situation, while inclosed in a sheath of gilt paper, to be only 3° less cold than the surrounding grass during a night favourable to the production of cold on the surface of the earth.

I collected only a few facts respecting the comparative temperatures of different metals when they were exposed together to the sky on dewy nights; but such as I did collect tend to prove that the most readily dewed metals become colder than the air, sooner than those which receive dew with greater difficulty.

Many of the experiments which have been mentioned in this section show that, when bodies which had been equally exposed to the night air were examined at the same time, those which were most dewed were also the coldest. No such correspondence, however, was found in the experiments of different nights, or even of different parts of the same night. Thus, during two nights on which grass was 12° and 14° colder than the air, there was little dew; while on the night which afforded the most copious dew ever observed by me, the cold possessed by the grass beyond that of the air was, for the most part, only 3° and 4°; and I have always seen less dew about sunset than about sunrise, when the weather has been calm and clear at both times, though there is commonly, in this country at least, a greater difference between the temperature of grass and of air in the evening than in the morning. I had early observed, also, bodies exposed to the sky, on a cloudy but calm night, to be sometimes 2° or 3° colder than the air, without having any appearance of dew; and when two metals possessing different relations to dew were exposed together, I have seen the one which was the fitter to attract that fluid, colder than the other, though both were dry.

I shall conclude this part of my Essay with relating the results of some experiments which were made for the purpose of ascertaining the tendencies of various bodies to become cold upon exposure to the sky at night. Unfortunately, the weather was not always favourable to my views; but what occurred appears to me, notwithstanding, worthy of being related.

In the observations hitherto given by me on the cold connected with dew, the temperature of grass has been chiefly considered, partly because my first experiments had been made upon it, and partly from a wish, which arose afterwards, to compare my own experiments with those of Mr. Six, which had been confined to that substance. I found it, however, very unfit to furnish the means of comparing the degrees of cold produced at night on the surface of the earth, at different times and places; as its state on different nights, on the same parts of the plat I commonly made use of, and in different parts of the plat on the same nights, was often very unequal in point of height, thickness, and fineness; all of which circumstances influenced the degree of cold produced by it.[25] I observed, in consequence, a much greater uniformity in the results of experiments made with various other bodies, whose condition, when first exposed to the air, was always the same. Of these the most productive of cold were the filamentous and downy—as wool of moderate fineness, very fine raw silk, very fine unspun cotton, fine flax, and swansdown, all of which were not only more steadily cold, upon clear and calm nights, than grass, but also gave rise to a greater degree of cold than was almost at any time observed upon it, even in its best state. Among the bodies of this class wool produced the least cold, and I formerly

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[25] Notwithstanding this assertion, and the confirmation given to it by the experiments made by Glaisher, Melloni, and, indeed, every meteorologist subsequent to Wells, the observations on nocturnal radiation from the earth's surface continue to be made by expositing naked thermometers on grass. It is in every respect highly desirable that some settled plan should be followed by meteorologists by which observations on radiation from the sun, or from the earth, may be obtained which will admit of comparison. "The readings of thermometers placed on grass being found so variable, the unfitness of it to furnish the means of comparing the degrees of cold at night on the surface of the earth was evident. A much greater uniformity was observed in the results of experiments made with other substances, which were bad conductors of heat, and whose condition was always the same."—Glaisher, *Phil. Trans.*, 1847.

Probably glass of similar quality to the material of the thermometer would give the desired uniformity in the method of observing, and would furnish comparable results. It might be used as a slab, 20 inches long, 10 wide, and a quarter of an inch thick, and kept clean on its upper surface.

mentioned that it attracted less dew than silk, cotton, and flax. The last-mentioned substances, and swansdown, were found equal, or nearly so, in their tendency to become cold. Swansdown, however, exhibited the greatest cold rather more frequently than any of the rest; on which account, and from its being more easily managed, as it was used while adhering to the skin of the bird, I at length scarcely ever employed any other body of the same class. On the night during which grass was observed to be 14° colder than the air, swansdown, lying upon a neighbouring piece of grass, was still one degree lower. This difference of 15° between the temperature, at night, of a body on the surface of the earth and that of the air, a few feet above the earth, is the greatest which I have hitherto seen.

Fresh unbroken straw and shreds of white paper, though not properly to be ranked among filamentous substances, were also found to be a little more productive of cold than the wool which I used.

The next class consisted of bodies in the state of a powder more or less fine. These were clean river sand, glass, chalk, charcoal, lampblack, and a brown calx of iron. Chalk produced the least, and the three last substances the greatest cold. They were, all, however, inferior in this respect to bodies of the first class.

Solid bodies having a surface exposed to the sky of at least 25 inches square formed a third class, on which such experiments were made. The particular substances of this description subjected to trial were glass, brick, cork, oak-wood, and wax; all of which were, likewise, found inferior to the filamentous substances. From these last experiments it follows that when a glass bulb of a thermometer is applied at night to a body exposed to a clear sky, the temperature exhibited by the instrument will not be accurately that of the body in question, except the disposition of the latter to become cold in such a situation be the same as that of glass. An example of this fact has been given in this Essay.*

My principal experiments, however, of this kind were made with snow.

On the 25th of January, 1813, the ground being then covered with snow about an inch deep, I went to my usual place of experiment in the country; but, during 8 hours that I attended to my thermometers, the whole sky was constantly overcast with clouds. The atmosphere was, for the greater part of that time, very still, and a thermometer on the snow was generally about 2° lower than another in the air. That this difference was not owing to evaporation was proved by the thermometer on the snow always rising from a half to a whole degree whenever the air was a little moved, and falling the same quantity as soon as a great stillness again took place.

I had no opportunity of renewing my observations upon snow before the beginning of the present year, 1814. The state of my health rendering it improper that I should incur much fatigue, or be long exposed to night air, I restricted myself to the making a few experiments in the large garden in Lincoln's Inn Fields. I went thither, for the first time, on the evening of the 4th of January, immediately after a considerable snowfall had ceased, wishing to begin my observations before any cold should arise on the snow's surface from exposure to the sky. This was desirable on another account; for Mr. Kirwan, in direct opposition to indisputable facts most clearly stated by Mr. Wilson, had said that the great cold observed by that gentleman on snow was occasioned by this substance having retained the temperature of the high region from which it had fallen.† The result of my enquiry was that the surface of the snow and

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* Page 35. † On Temperatures, p. 30.

the air 4 feet above it had precisely the same heat. The depth of the snow was 4 inches.

My next experiment took place on the evening of the 6th, the intervening day having been snowy. The sky was clear, but the air had a considerable motion. The heat of the atmosphere, at the height of four feet, was at 9½ h. 26 ; while that of the surface of the snow and of swansdown lying upon it was 22°. The depth of the snow was now about 5 inches.

On the 7th, a little after sunset, the heat of the air in the garden was 23°, that of the surface of snow 19°, but that of swansdown lying upon the snow only 15 . There was then a gentle breeze; some parts of the sky were covered with clouds, and the lower atmosphere was a little obscure. While the exposed surface of the snow was 19°, a part of its surface, which had been covered about 20 minutes with a piece of pasteboard, was 22 . Grass at the bottom of the snow was 31°, and the earth an inch beneath the grass, 32°.

After this, there was no fit time for observation until the 13th. The thermometers were exposed at 8 h. on the evening of that day, the sky being then without clouds; but the stars were not bright, and there was a perceptible motion in the air. At 8½ h. the temperature of the air was 22°½, that of the surface of the snow 13°, and that of swansdown lying on the snow 8°. At 9 h. the air was 23°½, snow 17°, and swansdown 15°. The sky being now, in great measure, covered with high thin clouds, my experiments ceased. At 10½ h. the sky was very bright, and the atmosphere very calm; but it was not then convenient to me to renew my observations. Had I repeated them at that time, I should probably have found a difference between the temperature of the swansdown and air several degrees more considerable than the one of 14°½, which had already occurred on this evening, and consequently greater than the greatest observed by Mr. Wilson between the temperatures of snow and of the atmosphere, which was one of 16°.

The next favourable evening was that of the 21st. Much snow having in the meanwhile fallen, its depth was now more than a foot. The thermometers were observed 5 times between 4 h. 15 m. and 4 h. 55 m. At 4 of those times the swansdown was 13°, and at one of them 13°½ colder than the air, the heat of which at the 4 first observations was 26°, and at the last 25°½. The temperature of the surface of the snow during the whole period of observation was 17°, and consequently 4 times it was 4°, and once 5°, less cold than that of the swansdown. The atmosphere was altogether free from clouds and nearly quite calm, but a good deal hazy.

Before another proper evening arrived my health became so infirm that I was obliged to relinquish this pursuit. I conclude, therefore, my account of it with two remarks. 1. If Mr. Wilson had been accustomed to examine the temperature of swansdown, or any similar substance placed upon snow, he would probably have observed a cold on the surface of the earth exceeding that of the atmosphere by 20° or more on the night of his actually observing an excess of 16°. 2. Since upon one evening, when the atmosphere was neither very clear nor very still, a difference of 14°½ was found by me between the temperatures of air and of swansdown, which is only ½ a degree less than the greatest difference I have ever observed between the same substances on the stillest and clearest nights in summer, a corroboration is hence derived of a conclusion made by Mr. Six from his experiments, that the greatest differences at night, in point of temperature, between bodies on the surface of the earth and the atmosphere near to it are those which take place in very cold weather.[26]

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[26] "When a radiator is exposed to a clear sky, it tends to keep a certain thermometric distance—if I may use the term—between its temperature and that of the surrounding air. This distance will depend upon the energy of the radiator, but it is to a great extent independent of the temperature of the air. Thus, M. Pouillet has proved that in the month of April, when the temperature of the air was 38°·5 F., swansdown fell by radiation to 25°·7; the whole chilling effect, therefore, was 12°·8. In the month of June, when the temperature of the air was 64°, the temperature of the radiating swansdown was 41°; the chilling of swansdown by radiation is here 13°, almost precisely the same as that which occurred in April. Thus, while the general temperature varies within wide limits, the *difference* of temperature between the radiating body and the surrounding air remains sensibly constant.

"These facts enabled Melloni to make an important addition to the theory of dew. He found that a glass thermometer, placed on the ground, is never chilled more than 3°·6 F. below an adjacent thermometer, with *silvered bulb*, which hardly radiates at all. These 3·6°, or thereabouts, mark the thermometric distance above referred to, which the grass tends to preserve between it and the surrounding air. But Six, Wilson, Wells, Parry, Scoresby, Glaisher, and others, have found differences of more than 18° F. between a thermometer on grass and a second thermometer hung a few feet above the grass. How is this to be accounted for? Very simply; according to Melloni, thus:—The grass blades first chill themselves by radiation, 3°·6 F. below the surrounding air: the air is then chilled by contact with the grass, and forms around it a cold aërial bath. But the tendency of the grass is to keep the above constant difference between its own temperature and that of the surrounding medium. It therefore sinks lower. The air sinks in its turn, being still further chilled by contact with the grass; the grass, however, again seeks to re-establish the former difference; it is again followed by the air, and thus, by a series of actions and reactions, the entire stratum of air in contact with the grass becomes lowered to a temperature far below that which corresponds to the actual radiative energy of the grass."—Tyndall, "*Heat as a Mode of Motion.*"

It does not appear that the observations recorded by Wells support Melloni's addition to the theory; and, indeed, before the notion of a "thermometric distance" can be accepted as a fact, the matter must be much more carefully observed and worked out.

PART II.

ON THE THEORY OF DEW.

DEW, according to Aristotle*, is a species of rain, formed in the lower atmosphere, in consequence of its moisture being condensed by the cold of the night into minute drops. Opinions of this kind respecting the cause of dew are still entertained by many persons, among whom is the very ingenious Mr. Leslie of Edinburgh†. A fact, however, first taken notice of by Gersten, who published his treatise on dew in 1733, proves them to be erroneous; for he found that bodies a little elevated in the air often become moist with dew, while similar bodies lying on the ground remain dry, though necessarily, from their position, as liable to be wetted by whatever falls from the heavens as the former.

Shortly after the appearance of Gersten's treatise, Musschenbroek made the remark, already mentioned in this Essay, that metals will be free from dew while other bodies attract it copiously. This philosopher contented himself with publishing his discovery; but his friend Dufay concluded from it that dew is an electric phenomenon, since it leaves untouched the bodies which conduct electricity, while it appears upon those which cannot transmit that influence. If dew, however, were to form on the latter only, its quantity would never be sufficiently great to admit its being distinctly seen; for the non-conductors, as soon as they became in the least moist, would be changed into conductors. Charcoal,

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* Meteor. Lib. 1. c. x. et De Mundo, c. iii.

† Relations of Heat and Moisture, p. 37 and 132.

too, it is now known, though the best solid conductor of electricity after the metals, attracts dew very powerfully; and, in the last place, contrary to the assertion of Dufay, dew frequently forms upon metals themselves.

Other authors have ascribed the production of dew to electricity, for reasons different to that from Dufay. But there are several considerations which seem to me to prove that no such opinion can be just. 1. When dew is produced in a clear atmosphere, the portion of air by which it is deposited must necessarily be unable, at that moment, to retain in a state of pellucid vapour all the moisture which it had immediately before held in that form. But I know of no experiment which shows that air, by becoming positively electrical, which is said to be its condition on the evenings during which dew is most abundant, is rendered less able than it had previously been to contain watery vapour in a state of transparency. 2. Bodies in similar circumstances, as far as electricity is concerned, acquire very different quantities of dew. Wool placed on the raised board, for example, attracted very much more dew than wool attached to the lower side of the same board, and even considerably more than the same substance freely suspended in the air and entirely exposed to the sky. 3. Dew forms in different parts of the night in quantities no way proportioned to the degrees of electricity found in the atmosphere at the same times. Thus, it is commonly more copious in the morning than in the evening, notwithstanding that the air is observed to be, in the latter season, more highly electrical than in the former. 4. I have several nights held a glass bottle, upon which dew was forming, close to the top of a Bennett's electrometer, which had been previously kept in a dry place; but I never saw the slips of gold leaf to move in consequence. It is very probable, however, that more refined experiments will show that electrical appearances attend the production of dew. These, perhaps, accompany every change in the chemical form of bodies. But the facts which have been stated seem sufficient to establish that any such appearances which may be hereafter remarked during the formation of dew must be considered as effects, and not as the cause, of the conversion of the watery vapour of a clear atmosphere into a fluid.

A remaining argument applies equally to all the theories which have hitherto been made public on the cause of dew. This is, that none of them include the important fact that its production is attended with cold; since no explanation of a natural appearance can be well founded which has been built without the knowledge of one of its principal circumstances. It may seem strange to many that neither Mr. Wilson nor Mr. Six applied this fact to the improvement of the theory of dew.[27] But, according to their view of the subject, no such

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[27] Wells might have added also, Boyle, Le Roi, Pictet and Prevost.

Le Roi, in 1752, by an experiment of singular simplicity, satisfied himself that dew was deposited by the air. He sealed in a glass bottle atmospheric air at a temperature of 77°, and he found that as the temperature of the day declined the interior of the bottle became bedewed, and he concluded that the quantity of invisible vapour suspended in the air must be dependent on the temperature, and that, by cooling the air sufficiently, some of the vapour would be deposited as moisture. He termed the temperature at which the deposition took place the *degré de saturation de l'air*. "Le Roi noticed that the point of saturation, or the *dew-point* as we now call it, was often, during the day, very near the temperature of the air; but at night, when the air was many degrees colder than during the day, it was natural to suppose that on some nights it would fall below its point of saturation; and that, when such was the case, all the *moisture* in excess of that proper to the air temperature would be precipitated and form dew, and thus prove that dew is really formed by condensation of the moisture of the air."—*Tomlinson.* Experimentally he obtained this result, and, moreover, made the important observation that, although the nocturnal temperature might fall below the point of saturation, a change of wind would sometimes prevent the formation of dew. His method of determining the dew-point was as follows:—

"I take water sufficiently cooled to precipitate the moisture of the air on the exterior surface of the vessel that contains it. I pour this water into a a large glass, very dry on the outside, and plunge into it the bulb of a use could have been made of it by them, as they held the formation of that fluid to be the cause of the cold observed with it. I had many years, as was formerly mentioned, held the same opinion; but I began to see reason not long after my regular course of experiments commenced to doubt its truth, as I found that bodies would sometimes become colder than the air without being dewed; and that, when dew was formed, if different times were compared, its quantity and the degree of cold which appeared with it were very far from being always in the same proportion to each other. The frequent recurrence of such observations at length converted the doubt of the justness of my ancient opinion into a conviction of its error, and at the same time occasioned me to conclude that dew is the production of a preceding cold in the substances upon which it appears. Wishing, however, to obtain proofs more striking in degree of the validity of these inferences than such as had been afforded to me by casual observation, while attending to other parts of my subject, I instituted the experiments which will be next related.

I had frequently remarked, early in the evening, a considerable degree of cold on substances exposed in calm weather to a clear sky, and I had also sometimes seen, early in the evening,

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thermometer, in order to note its temperature. I allow it to become warmer by half a degree, and then pour it into another goblet. If the outer surface of the globe be still bedewed, I allow the water to become warmer half a degree at a time, until I have seized the exact point above which there will be no further precipitation. This point is the point of saturation of the air. For example, on the 5th October, 1752, the temperature of the air was 13°R., at 5^0 the surface of a cooled glass became bedewed; above this point it remained dry; below it moisture from this air was precipitated upon it, and that in greater quantity as the temperature was reduced." This very ingenious method would now be deemed far too rude, but, in principle, it is the same as that employed for finding the dew-point by means of Daniell's or Regnault's hygrometer. Le Roi's account of his experiments is given in the *Memoires de l'Academie Royale des Sciences*, 1752.

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the raised board altogether dry while the grass was much moistened. I therefore determined to make the experiments in view on the raised board, and to commence them as soon as the sun should cease to shine upon it. The first day I went to the country for this purpose, the 19th of August, 1813, almost every circumstance was favourable to its completion. There had been no rain for three weeks, the wind was northerly, and the barometer was rising; all which indicated that the atmosphere contained little moisture. The air, too, was extremely still. The only appearance in the least unfavourable was, that the sky was not entirely free from clouds; but these were few, of small extent, thin, and high.

At 6 h. 25 m. immediately after the sun had ceased to shine upon the spot where my experiments were to be carried on, though the time of its setting was still 47 minutes distant, I placed upon the raised board 10 grains of wool and a small bag, made of the skin of a swan's breast with the down adhering, and stuffed with wool, the whole weighing nearly 5 drachms. On each of these substances the naked bulb of a small and delicate thermometer was laid. A similar thermometer with its bulb also naked was suspended in the air over the grassplat, at the same height with the board. Two thermometers were placed in other situations, as will be seen in the annexed table. After an exposure of 20 minutes the wool was 7° colder than the air, but the swansdown bag only 6°, no doubt in consequence of its comparatively great quantity of matter. Neither, however, had gained the least weight according to the scales employed by me, which were sensibly moved by the 16th of a grain. These observations were repeated several times during the following hour, as will be seen by the Table, at none of which, except the last, was either the wool or swansdown found in the least heavier than when first placed on the board. At this last observation the wool, though 9°½ colder than the air, was still without any increase in weight; but the swansdown, which was 1° colder than the wool, had gained half a grain. My experiments now properly ceased, but having suffered the thermometers, which had been placed on the wool and swansdown and in the air, to remain in those situations, I examined them again at 8 h. 45 m., that is, 2 h. 20 m. after they had been first exposed. The wool, which was still 9°½ colder than the air, had gained somewhat less than half a grain; and the swansdown, which was now 11°½ colder than the air, had gained 2 grains, including the half grain already mentioned. When these last observations were made, the sky was entirely cloudless and the atmosphere very calm.

TABULAR VIEW OF OBSERVATIONS
on the Evening of August 19, 1813.

	6h. 45m.	7h.	7h. 20m.	7h. 40m.	8h. 45m.
Heat of air 4 feet above the grass	60½°	60½°	59°	58°	54°
—— wool on the raised board	53½	54½	51½	48½	44½
—— swansdown on the same	54½	53	51	47½	42½
—— surface of the raised board	58	57	55½		
—— grassplat*	53	51	49½	49	42

Similar experiments made at the same place on the evenings of the 25th of August and 17th of September in the same year, had results which were also similar, but less in degree; the greatest difference between the temperature of wool or swansdown while they were without any increase of weight, and the temperature of the air, having been on the first of those evenings only 4°, and on the second only 5°. The reasons were in great measure, if not wholly, that a considerable part of the sky was covered with clouds, and that the air was commonly in that state of motion which is denominated a gentle breeze.

* In these experiments, contrary to what usually happens, the grass was almost constantly colder than the filamentous substances, although they were placed upon the raised board.

On the evening of my first experiments I had omitted to measure the heat of the raised board before the thermometers were placed upon it. This was attended to on the two latter evenings, on the first of which its upper surface was found at the commencement of the experiments, 4° warmer than the air; on the second, both it and the air were of the same temperature. Again, on the first of the latter evenings 10 grains of wool, to which 3 grains of water had been added, having been laid on the raised board near the thermometers, at the end of 45 minutes the parcel was found to have lost 2½ grains of moisture by evaporation, during the time that dry wool had become several degrees colder than the air.

A fourth experiment of this kind was made by me on the 7th of January, 1814, in the garden of Lincoln's Inn Fields, by placing 10 grains of wool on a sheet of pasteboard, which lay upon the snow. At the end of 35 minutes the wool was 5° colder than the air without possessing any additional weight.

Having thus shown the justness of my former conclusion that the cold observed with dew, is the previous occurrence, and, consequently, that the formation of this fluid has precisely the same immediate cause as the presence of moisture upon the outside of a glass or metallic vessel, when a liquid considerably colder than the air has been poured into it shortly before; I shall next apply this fact to the explanation of several atmospherical appearances.

I. The variety in the quantities of dew, which were found by me upon bodies of the same kind, exposed to the air during the same time of the night, but in different situations, is now seen to have been occasioned by the diversity of temperature which existed among them.

II. Agreeably to the opinion of Mr. Wilson and Mr. Six, the cold connected with dew ought always to be proportional to the quantity of that fluid; but this is contradicted by experience. On the other hand, if it be granted, that dew is water precipitated from the atmosphere, by the cold of the body on which it appears, the same degree of cold, in the precipitating body, may be attended with much, with little, or with no dew, according to the existing state of the air in regard to moisture; all of which circumstances are found actually to take place.

III. The formation of dew, indeed, not only does not produce cold, but like every other precipitation of water from the atmosphere, produces heat. I infer this, partly because very little dew appeared upon the two nights of the greatest cold I have ever observed on the surface of the earth, relatively to the temperature of the air, both of them having occurred after a long tract of dry weather; and partly from the most dewy night which I have ever seen, having been attended, during the greater part of it, with no considerable degree of cold. On this night, the difference between the temperatures of grass and of air was at first 7°½, the dew being then not very abundant. But, after the dew had become very abundant, the difference of those temperatures never exceeded 4 , and was frequently only 3°.

With the view of obtaining, though indirectly, some knowledge of the quantity of cold, which had been prevented, by the formation of dew, from appearing on the surface of the earth, in the night just spoken of, I made the following experiment. To 10 grains of wool having the same form and extension, as the parcels employed for the collection of that fluid, were added 21 grains of water, this being the quantity of moisture which had been attracted by 10 grains of wool, lying on the grassplat, in the space of 8 hours on that night. The wet wool having been then placed in a china saucer, laid on a feather-bed in a room, the door and windows of which were shut, its heat during the following 8 hours was, at frequent examinations, uniformly found to be about 4° less than that of a dry china saucer on the same bed; the temperature of the air in the room not having altered more than half a degree in the course of the experiment. At the end of the 8 hours, the wool still retained 2½ grains of moisture. If this quantity had also evaporated, the cold uniformly produced during the 8 hours would, in all probability, have been about 4°½. From this experiment, therefore, I think it may be inferred, that the mean quantity of cold, which was prevented, by the formation of dew, from appearing on the ground, during the night which has been mentioned, was also about 4°½. But, as the production of dew, during some parts of the night, was at a greater rate than that of 21 grains for 8 hours, 1 or 2 degrees may be added for those times, which will raise the effect of the dew in diminishing the appearance of cold during them to about 6°, on the supposition, which cannot be far from the truth, that dew had been attracted as copiously by the grass as by wool which lay upon it.

The less difference commonly observed between the temperatures of grass and of air, in the morning, than what occurs in the evening, is likewise to be, in part, attributed to a greater quantity of dew appearing in the former than in the latter season.

A more remarkable fact, deriving an explanation from the same source, is the greater difference which takes place in very cold weather, if it be calm and clear, between the temperatures of the air and of bodies on the earth, at night, than in equally clear and calm weather in summer; since, in very cold weather, any diminution of the temperature of a portion of air, in contact with a cold body, will be attended, in consequence of the well known relations of the atmosphere to moisture, with a much less formation of water, than an equal diminution would be in summer, supposing the air, before it touches the cold body, to be at both times equally near to its point of repletion with moisture. [28]

IV. In very calm nights, a portion of air, which comes in contact with cold grass, will not, when the surface is level, immediately quit it, more especially as this air has become specifically heavier than the higher, from a diminution of its heat, but will proceed horizontally, and be applied successively to different parts of the same surface. The air, therefore, which makes this progress, must at length have no moisture to be precipitated, unless the cold of the grass which it touches should increase. Hence in great measure is to be explained, why on such nights, as have been just mentioned, more dew was acquired by substances placed on the raised board, than by others of the same kind on the grass, though it began to form much sooner in the latter than in the former situation, those on the raised board having received air, which had previously deposited less of its moisture.

A reason is now also afforded, why a slight agitation of the atmosphere, when very pregnant with moisture, should increase the quantity of dew; since fresh parcels of air will hence be more frequently brought into contact with the cold surface of the earth, than if the atmosphere were entirely calm.

V. Dew, in agreement with the immediate cause which has been assigned by me for its production, can never be formed, in temperate climates, upon the naked parts of a living and healthy human body, during the night; since their

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[28] "The formation of dew is one of the circumstances which modify and check the refrigerating effect of radiation; for, as the vapour is condensed, it gives out the latent heat with which it was combined in its elastic form, and thus, no doubt, prevents an excess of depression which might in many cases prove injurious to vegetation. A compensating arrangement is thus established, which, while it produces all the advantages of this gentle effusion of moisture, guards against the injurious concentration of the cause by which it is produced."—Daniell's *Met. Essays*.

heat is never less in this season, in such climates, than that of the atmosphere. I have, in fact, never perceived dew on any naked part of my own body at night, though my attention was much occupied, for three years, with every thing relative to this fluid, and though I had been, during that period, much exposed to the night air. On the other hand, in very hot countries, the uncovered parts of a human body may sometimes, from being considerably colder than the air, condense the watery vapour of the atmosphere, and hence be covered with a real dew, even in the day-time.

VI. Hygrometers formed of animal or vegetable substances, when exposed to a clear sky at night, will become colder than the atmosphere; and hence, by attracting dew, or, according to an observation of Saussure,* by merely cooling the air contiguous to them, mark a degree of moisture, beyond what the atmosphere actually contains. This serves to explain an observation made by M. De Luc †, that in serene and calm weather, the humidity of the air, as determined by an hygrometer, increases about, and after sunset, with a greater rapidity than can be attributed to a diminution of the general heat of the atmosphere.

These examples are sufficient to show the value of the fact, that bodies become colder than the neighbouring air, before they are dewed, in explaining many atmospherical appearances. To this point, the investigation of the cause of dew might have been carried at any time since the invention of thermometers; but its complete theory could not possibly, in my opinion, have been attained, before the discoveries on heat were made, which are contained in the works of Mr. Leslie and Count Rumford.

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* Hygronometrie, p. 25.

† Introduction á la Physique Terrestre, II. 491.

The experience of most persons, respecting the communication of heat among bodies in the open air, is confined to what happens during the day; at which time, those that are situated near to one another are always found to possess the same temperature, unless some very evident reason for the contrary should exist. To many, therefore, it may appear incredible, that a perfectly dry body, placed in contact, on all sides, with other bodies of the same temperature with itself, shall afterwards, without undergoing any chemical change, become much colder than they are, and shall remain so for many hours; yet these circumstances are found to occur in substances attractive of dew when laid on the surface of the earth in a still and serene night, and are in perfect agreement with the doctrine of heat, now universally admitted to be just.[29]

To render this more easy of apprehension, let a small body which radiates heat freely, and possesses a temperature in common with the atmosphere, higher than 32°, be placed while the air is clear and still, on a slow conductor of heat lying on the surface of a large open plain, and let a firmament of ice be supposed to exist at any height in the atmosphere; the consequence must be that the small body will, from its situation, quickly become colder than the neighbouring air. For, while it radiates its own heat upwards, it cannot receive a sufficient quantity from the ice to compensate this loss;

[29] "The investigation of the phenomenon of dew developes the singular relations of radiant heat, and the laws by which caloric is communicated from one body to another. The vegetable world in particular opens a fertile and most interesting train of observation. Of the different grasses each draws from the atmosphere during the night a supply of dew to recruit its energies, dependent on its form and its peculiar radiating power. Every flower has a force of radiation of its own, subject to changes during the day and the night; and the deposition of moisture on it is regulated by the peculiar law which this radiating power obeys; and this power will itself be influenced by the aspect which the flower presents to the sky, unfolding to the contemplative mind the most beautiful examples of creative wisdom."—Harvey, *Enc. Met.* vol. V.

little also can be conveyed to it from the earth, as a bad conductor is interposed between them; and there is no solid or fluid except the air, to communicate it laterally either by radiation or conduction. This small body, therefore, unless it shall receive from the air nearly as much heat as it has emitted, which, considering the little that can be communicated from one part of the atmosphere to another, in its present calm state, must be regarded as impossible, will become colder than the air, and condense the watery vapour of the contiguous parts of it, if they should contain a sufficient quantity to admit of this effect. But events similar to these occur when dew appears in an open and level grass field during a still and serene night. The upper parts of the grass radiate their heat into regions of empty space, which consequently send back no heat in return; its lower parts, from the smallness of their conducting power, transmit little of the earth's heat to the upper parts, which, at the same time, receiving only a small quantity from the atmosphere, and none from any other lateral body, must remain colder than the air, and condense into dew its watery vapour, if this be sufficiently abundant, in respect to the decreased temperature of the grass.* [30]

* I have adopted in this explanation the hypothesis of M. Prevost, of Geneva, on the constant radiation of heat by bodies in contact with the atmosphere, even at the time that they are exposed to the influence of bodies warmer than themselves; as it appears to agree perfectly with all the phenomena of the communication of heat, which do not depend upon conduction. I shall hereafter make frequent use of this hypothesis.

[30] M. Prevost, in 1792, published a work entitled *Recherches Physico-Mecaniques sur la Chaleur,* in which he "distinctly recognises the fact that when at night the sky is clear the air is generally colder near the earth than at a higher elevation; that in spring and autumn it seldom freezes under a cloudy sky, and that often on a calm night, when a cloud passes over the zenith of the observer, the thermometer instantly begins to rise. He explains this on the doctrine of radiant heat, which he says, passes easily through the air, but that clouds are opaque for heat as well as for light,—they absorb both, or only allow it to pass slowly. The heat radiating from the earth traverses the pure

This subject may be further illustrated by a reference to what happens in the experiment which has been used to prove the reflection of cold.

In the simplest form of this experiment, a small body, the bulb of a thermometer, possessing the temperature of the atmosphere, is placed before a larger cold body, rendered equal in effect to one still larger by means of a concave metallic mirror. In this situation the small body radiates heat to the larger, without receiving an equivalent from it, and, in consequence, becomes colder than the air through which its heat is sent, notwithstanding that it is continually gaining some heat, both from the air which surrounds it and from the walls and contents of the apartment in which the experiment is made. Dew, therefore, would as readily form upon the thermometer in this experiment as it would upon one suspended in the open air at night, under a clear sky, provided that the two instruments were equally colder than the atmosphere, and that this was in both cases equally near to being replete with moisture.*

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atmosphere with facility, but is intercepted by clouds, which form a sort of vestment for the earth; receiving the radiant heat on their lower surfaces they become warm there, as a garment does close to the body, and restore to the earth more heat than transparent air can do. Meanwhile the surface of the cloud cools from the ease with which it emits heat into the rarefied air above."—*Tomlinson.*

This explanation of the effect of clouds is almost identical with that which a modern philosopher would give. Dr. Tyndall's experimental discovery that aqueous vapour is a good radiant and a good absorbent of heat, whereas dry air is devoid of these properties, would only serve to modify the language employed.

Harvey has a note in which these words occur (*Enc. Met.* vol. V. p. 128): "Above the clouds, however, the heavens are serene, and consequently radiation goes on freely from their upper surface. Hence the principal cause of the refrigeration upon which depends the formation of the nucleus of the hailstone." This is in reference to Volta's theory of hail; but, he adds—"It is proper to observe that this theory of the radiation of heat from the superior surfaces of clouds had already been proposed by Gay Lussac."

* The invention of this experiment having been ascribed a few years ago to M. Pictet, of Geneva, various English writers have shown that it occurs

Regarding now as established that bodies situated on or near to the surface of the earth become, under certain circumstances, colder than the neighbouring air, by radiating more heat to the heavens than they receive in every way,* I shall in the first place offer a few remarks on the extent and use of this occurrence, and shall afterwards apply the knowledge of it to the explanation of several more of the appearances described in the former part of this Essay, and of some others, which have not hitherto been mentioned by me.

Radiation of heat by the earth to the heavens must exist at all times; but, if the sun be at some height above the horizon, the degree of which is hitherto undetermined, and probably varies according to season, and several other circumstances, the heat emitted by it to the earth will overbalance, even in places shaded from its direct beams, that which the earth radiates upwards. I suspended at midday on the 24th of July, 1813, in the open air over a grassplat,

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in several much older foreign authors. But I have not seen any mention made of its having been also long since known in this country. That it was so appears from the following extract of a letter, written by Mr. Oldenburgh to Mr. Boyle, in 1665. "I met the other day in the Astrological Discourse of Sir Christopher Heydon, with an experiment, which he affirms to have tried himself, importing that cold accompanies reflected light, by employing burning spherical concaves, or parabolical sections, which, he saith, will as sensibly reflect the actual cold of snow or ice, as they will the heat of the sun." Boyle's Works, folio, vol. V. p. 345.

* Count Rumford offered the following conjecture, in a paper printed in the Philosophical Transactions for 1804. "The excessive cold which is known to reign, in all seasons, on the tops of very high mountains, and in the higher regions of the atmosphere, and the frosts at night, which so frequently take place on the surface of the plains below, in very clear and still weather, in spring and autumn, seem to indicate, that frigorific rays arrive continually at the surface of the earth, from every part of the heavens." But he gave no experiments to prove that such a communication actually exists between the heavens and the earth at night. Neither does it appear from any of his writings which I have seen that he ever supposed that the surface of the earth is more cooled by these frigorific rays, than the air through which they pass, or that some solid bodies are more cooled by them than others.

while the sky was wholly covered with very dense clouds and the weather calm, two delicate thermometers, one of which was naked, but the other cased in gold paper. At two observations, having an interval of 10 minutes between them, the thermometer in the gilt case was 2° lower than that which was naked. A white paper case was then drawn over the gilt one, upon which, after 5 minutes, the covered instrument was observed to be at the same height with the naked. The outer white case having, in the next place, been taken from the covered thermometer, but that which was gilt suffered to remain, the two instruments were in a few minutes found again to differ 2°. A thermometer on the grassplat was, during these experiments, higher than the naked instrument in the air by 2°, and than that in the gilt case by 4°. It is evident, therefore, that heat radiated by the sun must, on this day, have been transmitted in considerable quantity through the thickest clouds, since not only was the earth's surface warmer than the air, but a small body, covered with a substance not readily admitting the entrance of radiant heat, was colder than a similar body which was uncovered. In like manner I observed at noon on the 2nd of January, 1814, during the prevalence of a dense fog, a thermometer placed upon swansdown, which was lying upon grass thickly incrusted with hoarfrost, to be 2° warmer than the air, and 1° warmer than the grass.*

In a calm and serene night, however, when consequently little impediment exists to the escape by radiation of the earth's heat to the heavens, and when no heat can be radiated by the sun to the place of observation, an immense

* Another fact of the same kind, which occurred at the same time, is that although the temperature of the air was 30°, the hoarfrost on trees rapidly decreased, the solid matter of the trees intercepting radiant heat, which had penetrated through the fog from the sun, and converting it into heat of temperature.

degree of cold would occur on the ground, if the following circumstances did not combine to lessen it. 1. The incapacity of all bodies to prevent entirely the passing of heat by conduction from the earth to substances placed upon them. 2. The heat radiated to these substances by lateral objects. 3. The heat communicated to the same substances by the air. 4. The heat which is evolved during the condensation of the watery vapour of the atmosphere into dew.

The extent of the effect of all these checks upon the production of cold by the nightly radiation of heat from bodies on the surface of the earth, cannot in the present state of our knowledge be properly estimated; but facts show that notwithstanding their operation, the cold originating in this source must be often very considerable.

1. Mr. Wilson once observed a difference of 16° from this cause between the temperatures of snow and of air. In taking the latter temperature, however, he employed a naked thermometer, on which account, in consequence of what has already been mentioned to me, about 2° are to be added to the 16° noted by him, in order to obtain the real difference between the heat of the snow and the air at that time.*

2. If Mr. Wilson, as was formerly said, had laid a thermometer on any downy substance in contact with the snow, he would, in all probability, have found a cold indicated by it at least 20° greater than that of the air, as marked by a naked instrument, and consequently at least 22° greater than the real cold of the surrounding atmosphere.

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* As bright metals, when suspended in the air, and exposed to a clear sky on a calm night, become colder than the surrounding atmosphere, a thermometer covered with metalled paper, and placed in the circumstances which have been just mentioned, will mark a temperature less than that of the air near to it. But, as the difference must be small, and as I know of no way to estimate it accurately, I have hitherto always neglected to consider it.

3. Mr. Wilson's place of observation was not very favourable to the occurrence of a great cold from radiation of heat at night, it being near to a large smoky city in the immediate vicinity, also, as appears to me from what he says of it, of one or more considerable buildings, and in a climate abounding in moisture.

4. None of Mr. Wilson's experiments, in which a very great degree of cold occurred, were made within an hour or two after sunset, during which time according to my observation, the most considerable differences between the temperatures of the air and of bodies on the surface of the earth commonly happen.

If then such experiments should be made in an atmosphere still colder than that in which Mr. Wilson made his, on a large plain remote from any city, and free from objects of every kind that are elevated above the ground, and in a country remarkable for the dryness of its air, all which circumstances may be found in Russia during the winter; a difference of at least 30° would probably appear, on some still and serene night, between a small thermometer placed with its bulb naked* on the middle or leeward side of a stratum of a downy substance, occupying a space upon a grass field or bed of snow, one or two square yards in extent, and a similar thermometer inclosed in a case of gilt paper, and suspended in the air a few feet above the other. Two thermometers thus placed would, I think, be sometimes found even in this country to differ not much less than 30°. I have myself never made any such experiments with a downy substance which had a surface of more than a few square inches, or in a very cold night when the atmosphere was clear and calm, and the scene of observation remote from large masses of building.

* The effect would, perhaps, be a little increased, by covering the bulb with a very thin layer of lamp-black.

But even a cold of 30° appears not to be the greatest that can be thought to occur from the radiation of heat to the heavens, at night, by substances on the surface of the earth. For experiments by M. Pictet,* Mr. Six,† and I may add by myself, establish that in exception to the common rule, the heat of the atmosphere in clear and calm nights *increases* with the distance from the earth. Agreeably to Mr. Six's experiments the atmosphere at the height of 220 feet is often upon such nights, 10° warmer than what it is 7 feet above the ground.[31] If, therefore, I am able to show, as I expect I shall be in the course of a few pages, that the air at the smaller height becomes colder than that of the greater, from its vicinity to the surface of the earth, previously rendered cold by radiating its heat to the heavens, it will follow that these 10° must be added to the quantity of cold already mentioned; and, consequently, that a body on the ground may become at night at least 40° colder than the air two or three hundred feet above it, by the radiation of its heat to a clear sky.[32]

I shall add, with the greatest diffidence, a few words upon a final cause of the radiation of heat from the earth at

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* Essai sur le Feu, c. x.

† Phil. Trans. 1784, and 1788.

[31] The following observations made by Mr. Glaisher, in his balloon ascents, and published in the newspapers, will be read with interest in connection with Wells's statement.

July 23rd, 1863.—About 5 h. 40 m. p.m., at 600 feet the temperature of the air was 61°·5, there was no change in descending to the earth, where it was still 61°·5.

Dec. 1st.—About 2 h. 40 m. p.m., temperature 48°, remained unchanged till a height of 600 feet was passed.

June 13th, 1864.—About 8 p.m. at 2000 feet, 53°, no alteration in temperature in reaching the earth, which occupied 20 minutes.

Dec. 30th.—About 3 p.m. at 900 feet, 38°¼, no change at 300, and the same at the surface.

[32] "The reading of a thermometer on raw wool was 25° less, whilst another placed at 8 feet from the ground and fully exposed to the sky, was 3°·5.

night, and upon some of the circumstances which modify its action, though fully conscious of the danger of error which is always incurred in the attempt to appreciate the works of our Creator.

The heat which is radiated by the sun to the earth, if suffered to accumulate, would quickly destroy the present constitution of our globe.* This evil is prevented by the radiation of heat by the earth to the heavens during the night, when it receives from them little or no heat in return. But through the wise economy of means, which is witnessed in all the operations of Nature, the prevention of this evil is made the source of great positive good. For the surface of the earth having thus become colder than the neighbouring air, condenses a part of the watery vapour of the atmosphere into dew, the utility of which is too manifest to require my speaking of it. I may remark, however, that this fluid appears chiefly where it is most wanted, on herbage and low plants, avoiding in great measure, rocks, bare earth, and considerable masses of water.† Its production too, by another wise arrangement, tends to prevent the injury that might arise from its own cause; since the precipitation of water

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greater than that in air at the height of 4 feet and protected from radiation, and thus a difference of 28·5° existed between the readings of two thermometers, the one placed on raw wool, and the other in air at the height of 8 feet. This difference was the greatest that I have ever seen."—Glaisher, *Phil. Trans.*, 1847.

* Count Rumford says: "May it not be by the action of these (frigorific) rays that our planet is cooled continually and enabled to preserve the same mean temperature for ages, notwithstanding the immense quantities of heat that are generated at its surface by the continual action of the solar rays?"—*Phil. Trans.* 1804, p. 181.

† I have no direct observations for the foundation of this assertion concerning considerable masses of water. But I hold it, notwithstanding, to be just, because as soon as the surface of the water is in the least cooled by radiation, the particles composing it must fall downwards from their increased gravity and be replaced by others that are warmer. The whole mass, therefore, can never in the course of a single night be sufficiently cooled to con-

upon the tender parts of plants must lessen the cold in them which occasions it. I shall observe in the last place, that the appearance of dew is not confined to any one part of the night, but occurs during its whole course, from means the most simple and efficacious. For after one part of the air has deposited its moisture on the colder surface of the earth, it is removed in consequence of that agitation in the atmosphere which exists during its stillest states, and gives place to another having its quantity of water undiminished; and again, as the night proceeds, a portion of air which had before deposited all the moisture which circumstances at that time permitted, is rendered fit by the general increase of the cold of the atmosphere, to give out a fresh parcel when it comes anew into contact with the ground.

I. The first fact which I shall here attempt to explain is the prevention, either wholly or in part, of cold from radiation, in substances on the ground, by the interposition of any solid body between them and the sky. This evidently appears to arise in the following manner. The lower body radiates its heat upwards, as if no other intervened between it and the sky; but the loss, which it hence suffers, is more or less compensated by what is radiated to it, from the body above, the under-surface of which possesses always the same, or very nearly the same temperature as the air. In this way, therefore, is to be accounted for the warmth of the substances, which were sheltered from the sky by the raised board, the pasteboard roof, and the hollow cylinders of earth and pasteboard. In these examples the interposed substances cannot be supposed to have remitted more heat than they received. But in situations where large masses of bare solid matter

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dense into dew any great quantity of the watery vapour of the atmosphere. Besides, I have found that even a small mass of water, as will be more particularly mentioned in the last part of this Essay, sometimes acquires no weight from the reception of dew in the space of a whole night favourable to the formation of that fluid.

exist, which are warmer than the atmosphere, from the heat of the preceding day or other causes, a greater heat will be received by the exposed body than what is radiated by itself. For example, it seems certain to me that the houses surrounding Lincoln's Inn Fields had an influence upon my thermometers during my experiments there at night, beyond what arose from their merely returning a quantity of heat, equivalent to that which they received from the surface of the garden. It is not, however, absolutely requisite that a body should be itself exposed to the sky on a clear and calm night in order to become colder than the atmosphere; exposure to the influence of another body, so situated, is sufficient for the production of a slight degree of this effect. Thus, I have always found wool attached to the underside of my raised board on such a night to be a little colder than the air; and it has appeared to me a sufficient reason for the fact that the wool in this situation was, in some degree, exposed to the influence of grass, which had become considerably colder than the atmosphere by radiating its heat to the sky.

II. No direct experiments can be made to ascertain the manner in which clouds prevent or occasion to be small the appearance of a cold at night upon the surface of the earth greater than that of the atmosphere; but it may, I think, be firmly concluded, from what has been said in the preceding article, that they produce this effect, almost entirely by radiating heat to the earth in return for that which they intercept in its progress from the earth towards the heavens. For although, upon the sky becoming suddenly cloudy during a calm night, a naked thermometer, suspended in the air, commonly rises 2 or 3 degrees, little of this rise is to be attributed to the heat evolved by the condensation of watery vapour in the atmosphere, as was supposed by Mr. Wilson;* since, in consequence of the ceasing of that part of

* Edin. Phil. Trans. I. 157.

the cold indicated by the thermometer, which was owing to its own radiation to a clear sky, the temperature of the atmosphere may seem to increase 2°, or more, notwithstanding that it has received no real addition. Besides, the heat which is extricated by the condensation of vapour during the formation of a cloud must soon be dissipated; whereas the effect of greatly lessening, or preventing altogether, the appearance of a superior cold on the earth to that of the air, will be produced by a cloudy sky, during the whole of a long night.

Dense clouds near the earth must possess the same heat as the lower atmosphere, and will therefore send to the earth, as much, or nearly as much heat as they receive from it by radiation. But similarly dense clouds, if very high, though they equally intercept the communication of the earth with the sky, yet being, from their elevated situation, colder than the earth, will radiate to it less heat than they receive from it, and may, consequently, admit of bodies on its surface becoming several degrees colder than the air. In the first part of this Essay an example was given of a body on the ground becoming at night 5° colder than the air, though the whole sky was thickly covered with high clouds.*

Islands, and parts of continents close to the sea, being, by their situation, subject to a cloudy sky, will, from the smaller quantity of heat lost by them through radiation to the

* M. Prevost of Geneva, in his work on "Radiant Heat," p. 382, has already in this way, conjecturally, accounted for the effect of clouds in diminishing at night the cold of the atmosphere and of the surface of the earth; but he seems not to have known that their effect on the temperature of the latter is much greater than that which they produce upon the air. My explanation of this influence of clouds on the temperature of the surface of the earth during the night is a direct consequence from the facts which I had observed respecting the prevention of cold on the ground from radiation by the interposition of solid bodies between it and the heavens, and occurred to me in 1812. M. Prevost's work, indeed, was published in 1809, but I did not see it before the summer of 1813, when it was lent to me by his relation,

heavens at night, in addition to the reasons commonly assigned, be less cold in winter than countries considerably distant from any ocean.

III. Fogs, like clouds, will arrest heat, which is radiated upwards by the earth, and if they be very dense and of considerable perpendicular extent, may remit to it as much as they receive. Accordingly Mr. Wilson found no difference at night, in very foggy weather, between the temperature of the surface of snow and that of the air. Several observations by myself tend to confirm that of Mr. Wilson. An instance, however, as was formerly said, occurred to me of a difference at night of 9° between the temperatures of grass crusted over with hoarfrost, and of air, during a very dense fog. A fact, remarked by Mr. Leslie, respecting fogs, serves to explain this apparent anomaly. For it was found by that philosopher* from experiments made with his photometer, that in mists and low fogs the diminution of the sun's heat is small, when compared with what occurs when the sky is obscured by a dense body of clouds; and it will, I presume, be readily granted, that the same state of the atmosphere which allows the heat of the sun to pass copiously will also give a ready transit to heat radiated by the earth. Now there are several reasons for believing that the fog, during which grass was 9° colder than the air, did not ascend far above the ground. 1. The barometer had been falling for some days before, and it is a matter of common observation that great fogs seldom

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Dr. Marcet of London, who at the same time said that he believed there was no other copy of it in Great Britain, except one, which had been sent by himself to Edinburgh.

Note to second edition.] I did not know until after the first edition of this Essay was printed, that M. Prevost had published his opinion on the effect of clouds in preventing the occurrence of cold at night in the atmosphere and upon the surface of the earth, as early as 1792, in a work entitled "Recherches sur la Chaleur."

* On Heat and Moisture, p. 57.

occur, except it be high. 2. On the day preceding the observation, the air, after having been extremely foggy for nearly a week, had become clear enough to allow the sun's being distinctly seen during the whole of the afternoon, though there was still a sufficient obscurity in the lowermost parts of the atmosphere, to obstruct considerably the view of objects on the ground and very near to it. 3. On the day following the observation the fog was again much less, on the next it disappeared, and was succeeded by snow. It is to be mentioned, likewise, that on the evening in question the state of the grass, which was the subject of experiment, was unusually favourable to the production of cold; since, contrary to general experience, it was as cold as swansdown. If, then, the latter substance, from the much greater regularity of the appearances exhibited by it, be taken as the standard by which the occurrences of different nights are to be compared together, it will follow that the fog of which I am speaking, though it did not prevent, must have lessened, the production of cold from radiation. For, on the preceding evening, when there was little fog, the atmosphere being equally still on both, the difference between swansdown and the air was 12°; and on another, a fortnight after, the difference at the same place of observation between thermometers in the same situations, was 14°½, the air being now free from fog. If the atmosphere had been as still on this as on the former evenings, a greater difference would doubtless have been seen. I conclude, therefore, that fogs do not in any instance furnish a real exception to the general rule, that whatever exists in the atmosphere, capable of stopping or impeding the passage of radiant heat, will prevent or lessen the appearance at night of a cold on the surface of the earth, greater than that of the neighbouring air.

It follows also from what has been said in this article, that the water deposited upon the earth during a fog at night may sometimes be derived from two different sources, one of which is a precipitation of moisture from a considerable part of the atmosphere, in consequence of its general cold; the other, a real formation of dew, from the condensation, by means of the superficial cold of the ground, of the moisture of that portion of the air which comes in contact with it. In such a state of things, all bodies will become moist, but those especially which most readily attract dew in clear weather.* I have had no opportunity, however, of trying this conclusion by the test of observation since it occurred to me.

IV. When bodies become cold from radiation, the degree of effect observed must depend, not only on their radiating power, but in part also on the greater or less ease with which they can derive heat by conduction from warmer substances in contact with them. Thus grass on a clear and still night, was constantly colder, sometimes very much colder, than the gravel walk, though a small quantity of sand placed upon grass was always nearly as cold as this substance. In this case the difference in temperature between the gravel walk and sand evidently depended on the different quantities of heat, which they received from the parts beneath. A like reason is to be given for dew appearing in greater quantity on shavings of wood than on the same substance in a more dense and compact form; and for filamentous and downy substances becoming colder than all others, even than lampblack, which is placed by Mr. Leslie at the head of the best solid radiators of heat. For the lampblack exposed by me being about 2 lines in depth, possessed, in consequence, a fund of internal heat, which would more readily pass to its cold surface than the heat of the lower parts of the downy substances would to their upper surface.

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* The moisture observed at night by Musschenbroek, in Holland, and called by him dew, appears to me to have been of this kind. See this Essay, p. 5.

This subject is illustrated by the following experiment: On a dewy evening I depressed into soft garden mould a drinking glass, having a thick flat bottom, until its brim was upon a level with the surrounding earth, and at the same time placed a similar vessel, with its cavity also towards the sky, on the surface of the mould. In the morning the inside of the depressed glass was entirely dry, while that of the other was dewed. I then applied the bulb of a small thermometer to the inside of the bottom of each vessel, on which I found the heat of that part of the depressed one to be 56°, but of the same part of that which stood on the mould only 49°½. At this time the temperature of the air was 53°. The cause, therefore, was evident, both of the wetness of the first vessel and of the dryness of the second.

From this source also is to be derived the reason why the prominent parts of various bodies were observed by Mr. Wilson to be crusted with hoarfrost, while their more retired and massy parts were free from it.*

V. Bodies, exposed in a clear night to the sky, must radiate as much heat to it during the prevalence of wind, as they would do if the air were altogether still. But in the former case little or no cold will be observed upon them above that of the atmosphere, as the frequent application of warm air must quickly return a heat equal, or nearly so, to that which they had lost by radiation. A slight agitation of the air is sufficient to produce some effect of this kind; though, as has already been said, such an agitation, when the air is very pregnant with moisture, will render greater the quantity of dew, one requisite for a considerable production of this fluid being more increased by it than another is diminished.

VI. A small body, as a thermometer, suspended in the

* Paper in Phil. Trans. 1780.

air, will even in the calmest night exhibit but little cold from radiation, since it is continually exposed to the application of fresh parcels of warmer air, both from the progressive motion of this fluid, and from the downward motion produced in it by the superior gravity of such portions as have been cooled by contact with the suspended body. On the other hand, a thermometer upon a board, raised above the earth and possessing a surface of several square yards, will have its cold from radiation much less diminished than the former, as it is exposed to no loss from a downward motion of the air, and as the air, which approaches it horizontally, must, almost always, have had its temperature previously lowered by passing over another part of the board. The reason then of the lee side of the raised board being often colder than the windward is obvious.

VII. There is a remark by Theophrastus,* which has been confirmed by other writers, that the hurtful effects of cold occur chiefly in hollow places. If this be restricted to what happens on serene and calm nights, and it does not, I believe, hold true in any other circumstances, two reasons from different sources are to be assigned for it. The first is, that the air being stiller in such a situation than in any other, the cold, from radiation in the bodies which it contains, will be less diminished by renewed applications of warmer air; the second, that from the longer continuance of the same air in contact with the ground in depressed places than in others, less dew will be deposited, and therefore less heat extricated during its formation. It will be seen in the last part of this Essay, that in the East Indies depressions in the earth are artificially made for the purpose of increasing the cold which appears in serene nights. On this subject, however, it is to be observed, that if the depressed or hollow places be deep in

* Lib. v. c. xvi.

proportion to their horizontal extent, a contrary effect must follow; as a case will occur more or less similar to that which existed in some experiments formerly related by me, in which a small portion of grass was surrounded by a hollow cylinder.

VIII. An observation closely connected with the preceding, namely, that in clear and still nights frosts are less severe upon hills than in neighbouring plains,* has excited more attention, chiefly from its contradicting what is commonly regarded an established fact, that the cold of the atmosphere always increases with the distance from the earth. This inferior cold of hills is evidently a circumstance of the same kind with that ascertained by M. Pictet and Mr. Six respecting the increasing warmth, in clear and calm nights at all seasons of the year, of the different strata of the atmosphere, in proportion as these are more elevated above the earth. As the greater cold of the lower air is the less complicated fact, I shall attempt to explain it in the first place. M. Pictet, indeed, furnishes an explanation himself by ascribing it to the evaporation of moisture from the ground. But to show that this is not just, it need only be mentioned that the appearance never occurs in any considerable degree, except upon such nights as are attended with some dew, and that its great degrees are commonly attended with a copious formation of that fluid; since it cannot be thought that the same stratum of air will deposit moisture on the ground from an insufficiency of heat, at the very time it is receiving moisture from the ground in the state of pellucid vapour, as this presupposes that it is not yet replete with water.

Our atmosphere has been very generally regarded as incapable of being heated directly by the rays of the sun, prin-

* Theophrastus also remarks that it freezes less on hills than on plains, but without mentioning that this happens only on calm and serene nights. (Lib. v. c. xx.)

cipally because these give no heat to any particular portion of it, in which they are brought to a focus. I do not know whether this experiment was ever made with all the accuracy of which it is susceptible; but, granting that it has been thus made, my opinion is, notwithstanding, that no reliance can be placed in it. For as air, if heated at all by concentrated sunbeams, must be heated by them in a very slight degree during the time that their focus may be looked upon as stationary, otherwise the present question would not have arisen, it is necessary for conducting the experiment properly that, during the whole of it, the same individual small portion of air shall constantly receive that focus; but this, for various manifest reasons, cannot possibly happen. Viewing, therefore, the argument founded upon this experiment as without force, I shall now offer several considerations which seem to prove that air is actually heated by the sunbeams which enter it.

1. Air both reflects and refracts light, and all other bodies, as far as I know, acquire heat while they act thus on the light of the sun.

2. Air suffocates or absorbs the sun's light, which it cannot be supposed to do without increasing in temperature.

3. If air, considered as a uniform fluid, were even incapable of gaining heat directly from the sun's rays, heat would be communicated by them to it through the intervention of the innumerable particles of solid matter, which the trivial experiment of receiving a sunbeam into a darkened room shows to be present in the atmosphere. Should it be said that this appearance may occur only in the neighbourhood of the earth from the accidental admixture of solid matter raised from its surface by winds, or in any other way, the answer is, that as my inquiry is concerning the existence of a certain condition of the atmosphere, it matters not how this originates. Nothing more can be demanded than that

it should always be found, which I believe to be the case; since, if I can trust my memory with respect to what took place many years ago, I should say that such particles are to be seen, by means of the sun's light, in the air over the middle of the Atlantic ocean. These particles then must receive heat from the sunbeams, which impinge upon them, and this they will communicate to the contiguous pellucid air.

4. Unless it be admitted that the atmosphere is capable of intercepting part of the heat, which is radiated into it by the sun, and of converting this into heat of temperature, I deem it impossible to find a sufficient reason for the great warmth which exists, after a long calm, in air incumbent upon the Atlantic and Pacific oceans, at the distance of a thousand miles or more from any considerable body of land. It cannot be derived from the neighbouring water, since this is colder than the lower atmosphere; and no one will suppose it to be the same heat which the air had acquired from the last continent it had passed over many days before. But, if even this were supposed, another difficulty would remain to be removed, which is, that during the whole of the calm the air is cooled every night and again becomes warm in the day.* [33]

Should what has been said be thought sufficient to establish that the air arrests part of the sun's heat, which is radiated into it bound up with light, two consequences must

* One reason is hence apparent for the great coldness of the high regions of the atmosphere; since the air in them must be less fit than that of the lower strata to arrest heat which is radiated into it.

[33] Professor Tyndall has established, by experimental proof, the fact that dry air is as transparent to radiant heat as a vacuum; while aqueous vapour has a high absorbing power for radiant heat.

Glaisher has found, in our latitude, that at certain elevations the air is generally in a state almost absolutely free from aqueous vapour.

These results of modern researches will serve to considerably modify the arguments and reasoning of Wells on the subject of the heating of the atmosphere.

also be allowed. The first is that air will exert a greater power of the same kind upon heat radiated into it without light, since the sun's heat passes instantaneously through many bodies, which refuse a similar way to heat radiated by terrestrial substances; the other, that air must be as capable of becoming cold by radiating its own heat*, as of becoming warm from heat radiated into it, as these two properties are uniformly observed to exist together, and to be proportional to each other. [34] The truth of the latter conclusion may also be inferred from this fact, that in still and calm weather the heat of the air a few feet above the earth will sometimes decrease, even in this country, 18 or 20 degrees between sunset and sunrise, though no change of wind has in the meantime occurred; for the inconsiderable conducting power, which air is now known to possess, will permit only a small part of this diminution to arise from heat passing, by means of that power, from the atmosphere to the colder earth. Mr. Leslie †, indeed, ascribes this effect to the descent of cold air from the higher regions of the atmosphere; but if this were just, a less cold ought to be found on a clear and still night in the lower than in the higher strata, which is contrary to the uniform results of numerous experiments by M. Pictet and Mr. Six. Winds too, which produce such a mixture, always lessen the nocturnal decrease of temperature in the lowermost part of the atmosphere.

Having thus shown that air is capable, both of absorbing heat, which is radiated into it, and of radiating heat which had before formed a part of its temperature, I proceed to

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* M. Prevost says: "On peut supposer que les molecules de l'air rayonnent."—*Du Calorique Rayonnant*, p. 24.

† On Heat and Moisture, p. 11, and 132.

[34] Apply these conclusions to the aqueous vapour of the atmosphere instead of the "air," and they will receive the sanction of the best teaching of the present day.

apply the knowledge of these facts to the explanation of the phenomenon observed by M. Pictet and Mr. Six.

This phenomenon occurs on those nights only which permit bodies on the surface of the earth to become cold by radiating their heat to the heavens. On other nights when bodies thus situated were not colder than the air, I have observed the atmosphere within the limits of 9 feet from the ground, the boundary of my own experiments, to decrease a little in temperature, as the distance from the earth increased. Mr. Six likewise found that on cloudy nights the air was sometimes colder 220 feet above the ground than at the distance of 9 feet from it. When, therefore, the earth has become colder from radiation than the neighbouring air, in consequence of the latter having, by reason of its small radiating power, emitted a less proportion of its heat to the heavens, the warmer air must radiate a part of its heat to the earth, without receiving a full compensation, and will, therefore, become colder than it otherwise would have been. In proportion too as the air is nearer to the earth, must the cold of the former from this cause be the greater. My own conception of this matter is facilitated* by contemplating the occurrence of an opposite effect when the earth is warmer than the air. Let it be supposed then, that while the earth in this state, radiates upwards a quantity of heat, a foot in depth of the incumbent air is capable of stopping a 1000th of what it hence receives, and of converting it into heat of temperature. The consequence must be that the next foot, from receiving only 999 parts of what had been emitted by the earth, will not be so much heated as the first foot, though it should absorb the same proportional quantity of what enters it. In this way every successive foot will acquire a less quantity of heat than the preceding, and a state of the atmosphere be

* The same facility is afforded by considering cold as a body.

produced, like to that which is actually observed in a calm and sunny day.[35] In the day, however, the phenomena, from the heating of air by rays from the earth, are somewhat confused by the warmed portions rising upwards, and mixing with what is colder; whereas, at night, the air which has been cooled by radiating heat to the earth, is rendered, by an increase of gravity, the more fit to retain its low position. I have here for the sake of simplifying the argument, taken no notice of the cooling of any considerable mass of the air in consequence of the actual contact of its lowermost stratum with the earth, or by the conduction of the temperature of one portion of it to another. But, in a calm state of the atmosphere these effects must be inconsiderable, though it appears to me impossible, in the present state of our knowledge, to determine them with any precision.

According to the view which has been given by me of this subject, the heat of the air in a clear and calm night ought to increase within the limits of the phenomenon in some decreasing geometrical ratio as the atmosphere ascends; and this conclusion is so far confirmed by the observations of M. Pictet and Mr. Six taken together, that the increase of temperature is found to be greater in a given space very near to the earth, than in an equal space more remote from it.

To return to the immediate object of this article, the fact is certain, whatever may be thought of my explanation of it, that in every clear and still night the air near to the earth is colder than that which is more distant from it, to the height at least of 220 feet, this being the greatest to which Mr. Six's experiments relate. If then a hill be supposed to rise

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[35] The reader should compare this reasoning from theoretical considerations with the conclusions deduced by Tyndall from his most carefully conducted and remarkable series of experiments, given in his work, "Heat considered as a Mode of Motion."

from a plain to the height of 220 feet, having upon its summit a small flat surface covered with grass, and if the atmosphere during a calm and serene night be admitted to be 10° warmer there than it is near the surface of the low ground, which is a less difference, according to the observations of Mr. Six, than what sometimes occurs in such circumstances, it is manifest that should both the grass upon the hill and that upon the plain acquire a cold of 10° by radiation, the former will, notwithstanding, be 10° warmer than the latter.[36]

But the equality here supposed to be in the cold acquired by grass in two such situations can seldom exist. For according to an observation made by Aristotle,* and since frequently repeated, the air of high places is much more agitated than that upon low ground. The frequent renewal, therefore, from this cause, of the air in contact with the grass on the hill, will prevent it from ever becoming much colder than the general mass of the atmosphere at the same height. Consequently, any diminution in this way of the 10° of cold, formerly supposed to occur there from radiation, must be added to the difference of temperature in the grass in the two situations.

What has hitherto been said refers only to the occurrences on the very summit of the hill. With respect to its sides, these can be only a little colder than the atmosphere upon a level with them, even in its calmest state. For in the first place, they do not enjoy the full aspect of the sky; and, in the second, the air which is cooled by contact with them, will, from its increased gravity, slide down their declivity, and thus make room for the application of new and warm parcels to the same surface. The motion, too, thus excited in the air near to the sides of the hill, must

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[36] It may be doubted whether much reliance can be placed upon the results of Six's experiments at the height of 223 feet. The supposed case of the hill is certainly opposed to received notions.

* Meteor. lib. 1. c. x.

occasion a motion in that upon the summit, which may in some measure account for the last mentioned observation of Aristotle, as far as relates to what happens in a clear night.

The height of the hill in this example has been supposed to be small, to make it accord with that of the stations, whose temperatures were compared by Mr. Six with the heat of the air near the ground. But observations of the same kind will apply to hills of much greater elevation. For granting, first, that the air at the height of 220 feet is never more than 10° colder than that near to the earth, which is not probable, and is indeed contradicted by some of Mr. Six's observations; and again, that the increase of the air's heat in a calm and serene night, ceases precisely at the greatest height to which Mr. Six carried his observations, which is also improbable; still a reduction to the extent of 10° in the temperature of the air near to the earth, will render the cold of this low portion of the atmosphere greater than that of any other portion, which is not more than 2500 or 3000 feet above the former, if the estimate be just, which makes a declension in the heat of the atmosphere of 1° for every 250 or 300 feet of its height, when no counteracting cause exists.[37]

The remarks, however, which have been offered on the

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[37] The law of decrease of temperature for height above the earth's surface originated from the experiments made in balloons by Gay Lussac. It was generally received as a fact that for each 300 feet of elevation the temperature of the air was 1° lower than that at the surface. The balloon experiments made by Welsh in 1852 were even considered to confirm the law. The more recent, extensive, and careful meteorological observations made in balloon ascents by Glaisher, throw entire discredit upon this supposed law.

J. Glaisher, F.R.S., from a series of balloon ascents undertaken for meteorological research, deduced the following approximate law of decrease of temperature with elevation:—

When the sky was cloudy—

For the first 300 feet the decrease was 0°·5 for every 100 feet
From 300 to 3400 „ „ „ „ 0·4 „ „ „
„ 3400 to 5000 „ „ „ „ 0·3 „ „ „

greater warmth of hills at night in a certain state of weather, are strictly applicable to those only which are insulated and

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Therefore in cloudy states of the sky the temperature of the air decreases nearly uniformly with the height above the surface of the earth nearly up to the cloud region.

When the sky was partially cloudy—

In the first 100 feet there was a decline of				0°·9	
From 100 to 300	,,	,,	,,	0·8	for each 100ft.
,, 300 to 500	,,	,,	,,	0·7	,,
,, 500 to 900	,,	,,	,,	0·6	,,
,, 900 to 1800	,,	,,	,,	0.5	,,
,, 1800 to 2900	,,	,,	,,	0.4	,,
,, 2900 to 5000	,,	,,	,,	0·3	,,

The decline of temperature near the earth with a partially clear sky is nearly double of that with a cloudy sky; at elevations above 4000ft. the changes for 100 feet seem to be the same in both states of the sky.

At heights exceeding 5000 feet

	to 6000	feet the	decline	was	2°·8
thence to	7000	,,	,,	,,	2·8
,,	8000	,,	,,	,,	2·7
,,	9000	,,	,,	,,	2·6
,,	10000	,,	,,	,,	2·6
,,	11000	,,	,,	,,	2·6
,,	12000	,,	,,	,,	2·6
,,	13000	,,	,,	,,	2·5
,,	14000	,,	,,	,,	2·2
,,	15000	,,	,,	,,	2·1
,,	16000	,,	,,	,,	2·1
,,	17000	,,	,,	,,	1·9
,,	18000	,,	,,	,,	1·8
,,	19000	,,	,,	,,	1·8
,,	20000	,,	,,	,,	1·5
,,	21000	,,	,,	,,	1·3
,,	22000	,,	,,	,,	1·3
,,	23000	,,	,,	,,	1·0
,,	24000	,,	,,	,,	1·3
,,	25000	,,	,,	,,	1·1
,,	26000	,,	,,	,,	1·0
,,	27000	,,	,,	,,	1·0
,,	28000	,,	,,	,,	0·9
,,	29000	,,	,,	,,	0·8

Approximate Law of Moisture at Elevations.

With an overcast sky the degree of humidity being 77

At 1000 feet it was	77	At 4000 feet it was	80
,, 2000 ,, ,,	77	,, 5000 ,, ,,	83
,, 3000 ,, ,,	80	,, 6000 ,, ,,	82

When the sky was partially clear, the degree of humidity

On the ground was	63	At 4000 feet it was	76
At 1000 feet it was	68	,, 5000 ,, ,,	69
,, 2000 ,, ,,	77	,, 6000 ,, ,,	68
,, 3000 ,, ,,	76		

of inconsiderable lateral extent; and it is upon such chiefly, if not solely, that this phenomenon has been observed. The superiority of the cold of a low plain, from radiation, over that of a wide expanse of hilly ground will, for obvious reasons, be less; and no superiority of this kind will probably exist in the former situation, when the high ground is not only extensive, but flat on the top, forming what is called a table-land; unless, indeed, which seems to be actually the case, the air of such an elevated country should be commonly more agitated than that of lower places equally level.

An explanation may be now easily given of an observation by Mr. Jefferson, of Virginia,* which, however, had also been made by Aristotle† and Plutarch,‡ that dew is much less copious on hills than it is upon plains. For allowing, at first, the surface of the ground to be in both situations equally colder than the air which is near to it; still, as the production of dew must be in proportion to the whole depression of the temperature of the air which furnishes it, below what its heat had been in the preceding day, and as one part of this depression, the general cooling of the atmosphere is much more considerable on the plain than on the hill, moisture must necessarily be deposited more copiously

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Above the clouds the degree of humidity

at 7000 feet was	64	at 16000 feet was	45
„ 8000 „ „	58	„ 17000 „ „	33
„ 9000 „ „	52	„ 18000 „ „	21
„ 10000 „ „	52	„ 19000 „ „	36
„ 11000 „ „	48	„ 20000 „ „	33
„ 12000 „ „	48	„ 21000 „ „	32
„ 13000 „ „	43	„ 22000 „ „	21
„ 14000 „ „	58	„ 23000 „ „	16
„ 15000 „ „	53	„ 24000 „ „	unknown.

It would seem that at the higher elevations there is an almost entire absence of water.—*Vide Report of British Association*, 1862.

* Notes on Virginia, p. 132. † Meteor. Lib. 1. c. x.

‡ De Primo Frigido.

in the former than in the latter place. If the greater agitation of the atmosphere, and the less quantity of moisture during clear weather, in its higher region than in the lower be added, it may readily be inferred that dew shall sometimes be altogether wanting on a hill, though abundant on a plain at its foot, agreeably to what has been actually observed by Mr. Jefferson.

IX. The leaves of trees often remain dry throughout the night, while those of grass are covered with dew. As this is a similar fact to the smallness of dew on hills, I shall in accounting for it do little more than enumerate the circumstances on which it depends.

1. The atmosphere is several degrees warmer near the upper parts of trees on dewy nights than close to the ground. 2. The air in the higher situation is more agitated than that in the lower. 3. The air at a little distance from the ground, from being nearer to one of its sources of moisture, will on a calm evening contain more of it than that which surrounds the leaves of elevated trees. 4. Only the leaves of the very tops of trees are fully exposed to the sky. 5. The declension of the leaves from an horizontal position will occasion the air, which has been cooled by them, to slide quickly away and be succeeded by warmer parcels. 6. The length of the branches of the trees, the tenderness of their twigs, and the pliancy of the foot-stalks of their leaves, will cause in the leaves an almost perpetual motion, even in states of air that may be denominated calm. I have hence frequently heard, during the stillness of night, a rustling noise in the trees, which formed one of the boundaries of the ordinary place of my observations, while the air below seemed without motion.

Nearly in the same manner is to be explained why shrubs and bushes also receive dew more readily than lofty trees.

X. Bright metals, exposed to a clear sky in a calm night, will be less dewed on their upper surface than other solid bodies; since of all bodies they will, in such a situation, lose the smallest quantity of heat by radiation to the heavens, at the same time that they are capable of receiving, by conduction, at least as much heat as any others from the atmosphere, and more than any others from the warmer solid substances which they happen to touch.

If the exposed pieces of metal be not very small, another reason will contribute somewhat to their being later and less dewed than other solid substances. For, in consequence of their great conducting power, dew cannot form upon them, unless their whole mass be sufficiently cold to condense the watery vapour of the atmosphere; while the same fluid will appear on a bad conductor of heat, though the parts a very little beneath the surface are warmer than the air.*

From the same ready passage of heat from one part of a metal to another, a metallic plate suspended, horizontally, in the air several feet above the ground, will be found dewed on its lower side if the upper has become so; while the lower surface of other bodies more attractive of dew, but worse conductors of heat, are without dew in a similar situation.

A metal placed at night in the air near to the ground is, for the most part, sufficiently cold to condense, on its underside, the vapour which arises from the warmer earth, though its upper surface may be dry, from possessing the same, or almost the same temperature, as the atmosphere near to it.

As the temperature of metals is never much below that of the neighbouring air, a slight diminution of their cold from radiation will often occasion them to evaporate the dew which

* I hence think it probable that dew will sometimes form on the bulb of a thermometer, before the mercury in it is cooled below the temperature of the air. It seems certain to me, also, that dew may appear upon substances which, from the thinness of the layer of matter their cold is confined to, will produce little or no sensible effect upon a thermometer that is applied to them.

they had previously acquired, though other substances which had been more cooled by radiation are still attracting dew. For a like reason a metal, which has been purposely wetted, will often become dry at night, while other substances are becoming moist.

A substance highly attractive of dew, such as wool, if laid upon a metal, will derive heat from it, and will therefore acquire less dew than an equal portion of the same substance laid upon grass.

A large metallic plate will be less readily dewed while lying on grass than if it were placed in the air, though only a few inches above the grass, because, in the former situation it receives freely, by means of its great conducting power, heat from the earth; whereas, when placed in the air, it powerfully resists by another property, possessed in a great degree by bright metals, the entrance of heat radiated towards it by the grass beneath. Besides, the grass under the metal possesses now less heat than when this substance was in contact with it, partly from having a small oblique aspect of the sky, and partly from receiving air which has been cooled by passing over other grass fully exposed to the heavens.

When a piece of metal, having closely applied to its under surface a substance of some thickness, which attracts dew powerfully, and, therefore, imbibes readily heat that is radiated to it, is exposed to the sky at night, the heat supplied by the attached substance, both from its own original store and from what it has acquired through the radiation of the ground to it during the exposure, will enable this piece to resist longer than a bare piece the formation of dew, or even than another piece which has only a thin coat of matter considerably attractive of dew attached to its underside. The experiment with the wooden cross, covered with gilt paper, affords an example of the latter fact.

A very small metallic plate, suspended in the air, is less readily dewed than a large one similarly situated, as it receives, in proportion to its size, more heat from the atmosphere. On the other hand, a very small plate laid upon grass, rendered cold by radiation, will be sooner dewed than a larger one in the same situation, from presenting a greater proportional circumference to the surrounding grass, and therefore losing more quickly its heat by conduction. It will be also sooner dewed than another very small plate suspended in the air; since the latter, like other small bodies similarly placed, must be continually acquiring more heat than the former, in the manner described above in this Essay.*

A piece of metal, applied to different portions of cold grass in succession, will sooner become cold itself than another piece which is suffered to remain constantly upon one portion of the same grass, and will in consequence be sooner dewed.

If the bare side of a piece of metalled paper be exposed to a clear and calm sky at night, it will become cold by radiation, and receive by conduction, the heat of the inferior metallic surface; whence, if this surface be afterwards made the upper one, it will sooner acquire dew than a similar metallic surface which has been exposed to the sky during the whole of the experiment.

When a metal covers, in part only, the upper surface of a piece of glass, the uncovered portion of the glass quickly becomes cold by radiation, on exposure to a serene sky in a still night, and then, by deriving to itself a part of the heat of the metal, occasions this body to be more readily dewed than if the whole of the exposed surface had been metallic. In this experiment the outer edge of the metallic surface, from being nearest to the colder glass, will be the first and most dewed, while the parts of the uncovered glass, which are contiguous to the

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* Page 73.

warmer metal, will be the last and the least dewed of their respective substances.

A piece of glass, covered on one side with a metal, being placed on grass with this side down, its upper surface attracts dew as readily as if no metal were attached to it; since the metal in this situation has no power to lessen the radiation of heat from the upper surface of the glass. I conclude, however, from general principles, for I have not made the trial, that if the same piece of glass, having its metallic side still undermost, were raised in the air a little above the grass, it would be more readily dewed on its upper surface than if it had been without a metallic coating on the lower, as this coating must resist the introduction of heat radiated by the warmer grass, and thus preserve nearly undiminished the cold acquired from radiation of heat to the sky by the bare upper surface.

The proceding remarks apply to the whole class of metals; but the discoveries of Mr. Leslie, respecting the difference in the capacities of these bodies to radiate heat, furnish an explanation of a diversity among themselves in regard to attraction for dew, which was noted in the foregoing part of this Essay. Gold, silver, copper and tin, are there said to resist the formation of dew more strongly than other substances of the same class; but these metals, according to Mr. Leslie, radiate heat the most sparingly. On the other hand, lead, iron, and steel, which, according to the same author, radiate heat more copiously than the former metals, were found by me to acquire dew more readily. I do not know if the radiating power of platina has been ascertained by direct experiments; but, as its conducting power is small, its radiation must be great, since these qualities exist always in opposite degrees in the same substance; and I have accordingly observed it to be dewed while the four first mentioned metals were dry. I am ignorant both of the radiating and the conducting power of zinc, as determined by ordinary experiments; but I infer, from its being more easily dewed than gold or silver, that it radiates heat more copiously than they do; unless indeed, the pieces which I used, from having had their surfaces roughened by friction with sand, which was employed to brighten them, had acquired a radiating power greater than that possessed by polished pieces, agreeably to the results of some of Mr. Leslie's experiments.*

XI. Thinking it probable that black bodies might radiate more heat to the sky at night than white, I placed upon grass, on five different evenings, equal parcels of black and white wool. On four of the succeeding mornings the black wool was found to have acquired a little more dew than the white, whence I inferred that it had, in consequence of its colour, radiated a little more heat. But I afterwards remarked that the white wool was somewhat coarser than the black; which circumstance alone was sufficient to occasion a difference in their quantities of moisture. Another night I laid on the raised board a piece of pasteboard covered with white paper, and close to this a second piece, similar to the

* I once intended to subjoin here an explanation of some very curious observations by M. Benedict Prevost on dew, which were published, first in the 44th volume of the French Annals of Chemistry, and afterwards by M. Peter Prevost, of Geneva, in his Essay on Radiant Heat; but fearing to be very tedious, I have since given up the design. I will say, however, that if to what is now generally known on the different modes in which heat is communicated from one body to another, be added the two following circumstances: that substances become colder, by radiation, than the air, before they attract dew; and that bright metals, when exposed to a clear sky at night, become colder than the air much less readily than other bodies; the whole of the appearances observed by M. Prevost may be easily accounted for.

Note to Second Edition.—I found, shortly after the publication of the former edition of this Essay, that the learned Dr. Young had, several years before, in his great work on Natural Philosophy, employed the principle of the radiation of heat to account for several of the facts observed by M. B. Prevost. On the subject of Dr. Young's explanation, I have spoken somewhat fully in the 28th number of Dr. Thomson's Annals of Philosophy.

former in every respect, except that it was covered with paper blackened with ink. At daylight I saw hoarfrost upon both pieces, but the black seemed to have a greater quantity than the white. A doubt, however, afterwards arose upon the accuracy of this experiment likewise, for, as the light was faint when I viewed the two surfaces, the quantity of hoarfrost, though equal on both, might have appeared greater on the black than on the white, from the contrast of its colour with that of the former surface. But trials of this kind, as Mr. Leslie* has observed, never afford firm conclusions, since a black body must always differ from a white in one or more chemical properties, and this difference may of itself be competent to produce a diversity in their powers to radiate heat.

With the view to render the subject less complicated, I have hitherto treated of dew, as if it were altogether derived from watery vapour previously diffused through the atmosphere; this appearing to me to be by far its most considerable source, and none of my conclusions of any importance being liable to be effected, even by the establishment of a contrary opinion. Other writers, however, have regarded dew as being entirely the product of vapour emitted during the night by the earth and plants upon it. According to this theory dew is said to *rise*.

The first trace which I have found of the opinion that dew rises from the earth at night, occurs in the History of the Academy of Sciences for 1687. It is mentioned there briefly and obscurely, and was, probably, shortly forgotten, for Gersten, who advanced it anew in 1733, held himself to be its author. Musschenbroek and Dufay embraced it immediately after Gersten, but the former soon admitted that

* On Heat, p. 95.

dew sometimes *falls*. As far as I have learned, no writer upon dew has since ascribed its total production to vapour, emitted by the earth at night, except Mr. Webster of New England.* But this opinion is frequently advanced in conversation by persons not much accustomed to philosophical pursuits, chiefly, I think, because it contradicts a popular belief.

The only argument used by the French academicians in support of their opinion is, if I understand it rightly, that as much dew is observed under an inverted glass-bell as in any other situation. But admitting, for a moment, this to be true, they would not thus prove that the ground is the only source of that fluid.

Gersten was led to think that dew rises from the earth, by often finding grass and low shrubs moistened with it, while trees were dry. Respecting this fact, I shall add nothing to what I have lately said upon it. But his chief argument is derived from another fact related in the first part of this Essay, which is, that a plate of metal, laid upon bare earth on a dewy night will remain dry on its upper surface, while it becomes moist on the lower. This also is easily explicable by what has already been mentioned by me. For the lower side of the metal, in consequence of the upper being in contact with the air and being exposed to a clear sky, is colder than the earth a little below the surface, and therefore condenses the vapour, which strikes against its bottom; while the upper side, from being frequently warmer, and never more than a little colder than the air, is for the most part unable to condense the watery vapour of the atmosphere.† Gersten, moreover, describes several appearances himself which refute his opinion. He mentions, for

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* Mem. of American Acad. vol. III.

† I have, in like manner, observed, on a cloudy night, a piece of glass, laid over an earthen pan containing water and placed upon the ground, to

example, that the higher parts of shrubs are more dewed than the lower; that metallic plates, placed horizontally in the air, are as much dewed on their superior as on their inferior surfaces; and that convex and cylindrical bodies, suspended in the air, the latter having a position parallel to the horizon, are dewed only on their upper parts.

The principal reason given by Dufay for the rising of dew is, that it appears more early on bodies near to the earth than on those which are at a greater height. But this fact readily admits of an explanation on other grounds, that have already been mentioned. 1. The lower air, on a clear and calm evening, is colder than the upper, and will, therefore, be sooner in a condition to deposit a part of its moisture. 2. It is less liable to agitation than the upper. 3. It contains more moisture than the upper, from receiving the last which has risen from the earth, in addition to what it had previously possessed in common with other parts of the atmosphere. Dufay attempted to strengthen his argument by exposing, on three dewy nights, similar substances at different heights from the ground, expecting that the lower would always acquire more moisture than the upper; but, upon all the nights some one of the lower substances acquired less moisture than some one of the higher.

Mr. Webster has advanced no new fact in favour of the opinion of which I am speaking.

Enough having been said to prove that dew is not entirely the product of vapour rising from the earth at night, I shall next show that it often occurs when this cause can have little or no operation.

1. It appears from Hasselquist and Bruce that in Egypt, shortly before the rising of the Nile, and consequently when

be wet on its lower side, while the upper was dry, the glass being, in this situation, sufficiently cold to condense the vapour of water heated by the earth, but not enough so to condense the watery vapour of the atmosphere.

the ground there is in its driest state, dew becomes exceedingly plentiful, though little or none had formed before, while the earth was somewhat less dry. The cause evidently is, as was formerly mentioned, the moist air brought from the Mediterranean by the north wind which then prevails.

2. Mr. Webster, speaking of hoarfrost, which he properly regards as frozen dew, candidly says, though it overthrows his opinion: "This frost appears when the surface of the earth is sealed with frost, and of course the vapour of which it is formed cannot at the time perspire from the earth."

3. I have myself at all seasons of the year frequently observed wool upon the middle of the raised board, and, therefore, out of the way of vapour rising from the ground, to acquire more dew than wool laid upon the grassplat.

4. The bodies that condense the rising vapour must necessarily be colder than it; but, as they are likewise, according to the opinion under view, of the same temperature with the air surrounding them, this also should condense the rising vapour. Dew, therefore, should never appear in any considerable quantity, without being accompanied with fog or mist. Now I can assert after much attention to this point, that the formation of the most abundant dew is consistent with a pellucid state of the atmosphere. Hasselquist makes a similar observation with regard to Egypt; where, during the season remarkable for the most profuse dews, "the nights," he says, "are as resplendent with stars, in the midst of summer, as the lightest and clearest winter nights in the north."

But, although these facts prove that copious dews may occur with little or no contribution by vapour immediately rising from the earth, it must yet be admitted that some of the moisture which forms during clear and still weather on bodies situated upon or near its surface is in most cases to be attributed to this source; since, in my experiments, substances on the raised board became much later moist than others on the ground, though equally cold with them. The quantity from this cause, however, can never be great. For, in the first place, until the air be cooled, by the substances attractive of dew with which it comes in contact, below its point of repletion with moisture, it will be always in a condition to take up that which has been deposited upon grass, or other low bodies, by warm vapour emitted by the earth; just as the moisture formed upon a mirror by our breath is, in temperate weather, almost immediately carried away by the surrounding air. Accordingly, I have sometimes in serene and still weather observed dew to appear sparingly upon grass in the shade several hours before sunset, and to continue in nearly the same quantity till about sunset, when it would increase considerably at the time that the same fluid began to show itself on the raised board. In the second place, though bodies situated on the ground after they have been made sufficiently cold by radiation to condense the vapour of the atmosphere will be able to retain the moisture, which they acquire by condensing the vapour of the earth; yet, before this happens, the rising vapour must have been greatly diminished by the surface of the ground having become much colder. These considerations, added to the fact that substances on the raised board attracted rather more dew throughout the night than similar substances lying on the grass, warrant me to conclude that on nights favourable to the production of dew, only a very small part of what occurs is owing to vapour rising from the earth; though I am acquainted with no means of determining the proportion of this part to the whole. On the other hand, however, in a cloudy night all the dew that appears upon grass may sometimes be attributed to a condensation of the earth's vapour; since I have several times in such nights remarked the raised board to be dry, while the grass was moist. These nights were calm, and evaporation from the grass consequently not copious. When evaporation on cloudy nights was assisted by wind, dew has never, as was mentioned in the first part of this Essay, been anywhere observed by me.*

Agreeably to another opinion, the dew found upon growing vegetables is the condensed vapour of the very plants on which it appears. But this also seems to me erroneous, for several reasons. 1. Dew forms as copiously upon dead as upon living vegetable substances. 2. The transpired humour of plants will be carried away by the air which passes over them when they are not sufficiently cold to condense the watery vapour contained in it; unless, which is almost never the case if mist does not already exist, the general mass of the atmosphere be incapable of receiving moisture in a pellucid form. Accordingly, on cloudy nights when the air, consequently, can never be cooled more than a little below the point of repletion with moisture, by bodies in contact with it, dew is never observed upon any plants that are elevated a few feet above the ground. 3. If a plant has become, by radiating its heat to the heavens, so cold as to be enabled to bring the air in contact with it below the point of repletion with moisture, that which forms upon it, from its own transpiration will not then, indeed, evaporate. But other moisture

* The interval between the first appearance of dew in the afternoon on grass, in shaded places, and sunset, was formerly said by me on the authority, however, of only a few observations, to be considerably greater than that between sunrise and the ceasing of the formation of dew upon grass in the morning. These observations were made on spots exposed during the greater part of the day to the sun. In such places the heat acquired from the sun, by the uppermost layer of earth, will be longer retained than that acquired by the grass, which will, therefore, be sufficiently cool soon after the heat of the day has declined to condense a part of the vapour then copiously rising from the earth, whereas in the morning both less vapour will rise, the surface of the earth having now lost a great part of its heat, and a less proportion of that which does rise will be condensed by the grass, as the temperature of this body now more nearly approaches that of the ground, from first receiving the heat of the sun reflected from the atmosphere and other substances.

will, at the same time, be communicated to it by the atmosphere; and when the difference in the copiousness of these two sources is considered, it may, I think, be safely concluded that almost the whole of the dew, which will afterwards form on the plant, must be derived from the air; more especially when the coldness of a clear night, and the general inactivity of plants in the absence of light, both lessening their transpiration, are taken into account.

An experiment, however, has been appealed to in proof, that the dew of plants actually does originate from fluid transpired by them; that, namely, in which a plant, shut up in an air-tight case, becomes covered with moisture. But this experiment, if attentively examined, will be found to have little weight. First; the inclosed plant, being exempt from the cold, which its own radiation would have produced in its natural situation, on a dewy night, will transpire a greater quantity of fluid, than a similar plant exposed at the same time to the open air. Again; the small quantity of air contained in the case, must soon be replete with moisture, after which the whole of what is further emitted by the plant will necessarily assume the form of a fluid, whatever may be the condition of the external atmosphere; whereas, during even the clearest night, only a part of the smaller quantity of moisture emitted by the exposed plant, will be condensed on its surface. In the last place, notwithstanding the circumstances which favour the appearance of moisture upon inclosed plants from their own transpiration, still the quantity observed on them is said to be, for I have made no experiment myself respecting this matter, much less considerable than what is seen upon plants of the same kind, exposed to the air for the same time, during a calm and serene night.

PART III.

OF SEVERAL APPEARANCES CONNECTED WITH DEW.

There are various occurrences in nature which seem to me strictly allied to dew, though their relation to it be not always at first sight perceivable. The statement and explanation of several of these will form the concluding part of the present Essay.

I. I observed one morning in winter that the insides of the panes of glass in the windows of my bedchamber were all of them moist, but that those which had been covered by an inside shutter during the night were much more so than others which had been uncovered. Supposing that this diversity of appearance depended upon a difference of temperature, I applied the naked bulbs of two delicate thermometers to a covered and uncovered pane, on which I found that the former was 3° colder than the latter. The air of the chamber, though no fire was kept in it, was at this time 11°½ warmer than that without. Similar experiments were made on many other mornings, the results of which were: that when the warmth of the internal air exceeded that of the external, from 8° to 18°, the temperature of the covered panes would be from 1° to 5° less than that of the uncovered; that the covered were sometimes dewed while the uncovered were dry; that at other times both were free from moisture; that the outsides of the covered and uncovered panes had similar differences with respect to heat, though not so great as those of the inner surfaces; and that no variation in the quantity of these differences was occasioned by the weather's being cloudy or fair, provided the heat of the internal air exceeded that of the external equally in both of those states of the atmosphere.

The remote reason of these differences did not immediately present itself. I soon, however, saw that the closed shutter shielded the glass, which it covered, from the heat, that was radiated to the windows by the walls and furniture of the room, and thus kept it nearer to the temperature of the external air, than those parts could be, which, from being uncovered, received the heat emitted to them by the bodies just mentioned.

In making these experiments I seldom observed the inside of any pane to be more than a little damped, though it might be from 8° to 12° colder than the general mass of the air in the room; while, in the open air, I had often found a great dew to form on substances only 3° or 4° colder than the atmosphere. This at first surprised me; but the cause now seems plain. The air of the chamber had once been a portion of the external atmosphere, and had afterwards been heated, when it could receive little accession to its original moisture. It consequently required being cooled considerably, before it was even brought back to its former nearness to repletion with water; whereas the whole external air is commonly, at night, nearly replete with moisture, and therefore readily precipitates dew on bodies only a little colder than itself.

When the air of a room is warmer than the external atmosphere, the effect of an outside shutter on the temperature of the glass of the window will be directly opposite to what has been just stated, since it must prevent the radiation into the atmosphere of the heat of the chamber transmitted through the glass.

II. Count Rumford* appears to have rightly conjectured

* Phil. Trans. 1804. p. 182.

that the inhabitants of certain hot countries, who sleep at nights on the tops of their houses, are cooled during this exposure by the radiation of their heat to the sky; or, according to his manner of expression, by receiving frigorific rays from the heavens. Another fact of this kind seems to be the greater chill which we often experience upon passing, at night, from the cover of a house into the open air, than might have been expected from the cold of the external atmosphere. The cause, indeed, is said to be the quickness of transition from one situation to another. But, if this were the whole reason, an equal chill would be felt in the day, when the difference in point of heat between the internal and external air was the same as at night, which is not the case. Besides, if I can trust my own observation, the feeling of cold from this cause is more remarkable in a clear than in a cloudy night, and in the country than in towns. The following appears to be the manner in which these things are chiefly to be explained.

During the day our bodies while in the open air, although not immediately exposed to the sun's rays, are yet constantly deriving heat from them, by means of the reflection of the atmosphere. This heat, though it produces little change on the temperature of the air which it traverses, affords us some compensation for what we radiate to the heavens. At night, also, if the sky be overcast, some compensation will be made to us, both in towns and in the country, though in a less degree than during the day, as the clouds will remit towards the earth no inconsiderable quantity of heat. But on a clear night, in an open part of the country, nothing almost can be returned to us from above, in place of the heat which we radiate upwards. In towns, however, some compensation will be afforded, even on the clearest nights, for the heat which we lose in the open air, by that which is radiated to us by the surrounding buildings.

To our loss of heat by radiation, at times that we derive little compensation from the radiation of other bodies, is probably to be attributed a great part of the hurtful effects of the night air. Descartes* says that these are not owing to dew, as was the common opinion of his cotemporaries, but to the descent of certain noxious vapours, which having been exhaled from the earth during the heat of the day, are afterwards condensed by the cold of a serene night. The effects in question certainly cannot be occasioned by dew, since that fluid does not form upon a healthy human body in temperate climates; but they may, notwithstanding, arise from the same cause that produces dew on those substances, which do not, like the human body, possess the power of generating heat, for the supply of what they lose by radiation or any other means.

III. I had often, in the pride of half knowledge, smiled at the means frequently employed by gardeners to protect tender plants from cold, as it appeared to me impossible that a thin mat, or any such flimsy substance, could prevent them from attaining the temperature of the atmosphere, by which alone I thought them liable to be injured. But, when I had learned that bodies on the surface of the earth become, during a still and serene night, colder than the atmosphere, by radiating their heat to the heavens, I perceived immediately a just reason for the practice, which I had before deemed useless. Being desirous, however, of acquiring some precise information on this subject, I fixed, perpendicularly, in the earth of a grassplat, 4 small sticks, and over their upper extremities, which were 6 inches above the grass, and formed the corners of a square, the sides of which were 2 feet long, drew tightly a very thin cambric handkerchief. In this disposition of things, therefore, nothing existed to prevent the free passage of air from the exposed grass to that which was

* Meteorolog. c. vi.

sheltered, except the 4 small sticks, and there was no substance to radiate heat downwards to the latter grass, except the cambric handkerchief. The temperature of the grass, which was thus shielded from the sky, was upon many nights afterwards examined by me, and was always found higher than that of neighbouring grass which was uncovered, if this was colder than the air. [38] When the difference in temperature between the air several feet above the ground and the unsheltered grass did not exceed 5°, the sheltered grass was about as warm as the air. If that difference, however, exceeded 5°, the air was found to be somewhat warmer than the sheltered grass. Thus, upon one night, when fully exposed grass was 11° colder than the air, the latter was 3° warmer than the sheltered grass; and the same difference existed on another night, when the air was 14° warmer than the exposed grass. One reason for this difference, no doubt, was that the air, which passed from the exposed grass, by which it had been very much cooled, to that under the handkerchief, had deprived the latter of part of its heat; another, that the handkerchief, from being made colder than the atmosphere by the radiation of its upper surface to the heavens, would remit somewhat less heat to the grass beneath than what it received from that substance. But still, as the sheltered grass, notwithstanding these drawbacks, was upon one night, as may be collected from the preceding relation, 8°, and upon another 11°, warmer than grass fully exposed to the sky, a sufficient reason was now obtained for the utility of a very slight shelter to plants, in averting or lessening injury from cold on a still and serene night.

In the next place, in order to learn whether any difference would arise from placing the sheltering substance at a much

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[38] Patrick Wilson had previously written:—"I fastened with packthread a piece of open gauze to a hoop of 8 inches in diameter, and an inch deep; and, when the thermometers were sheltered in this manner, the quicksilver commonly rose nearly 2°."

greater distance from the ground, I had 4 slender posts driven perpendicularly into the soil of a grass field, and had them so disposed in other respects that their upper ends were 6 feet above the surface, and formed the angular points of a square having sides 8 feet in length. Lastly, over the tops of the posts was thrown an old ship flag of a very loose texture. Concerning the experiments made by means of this arrangement of things, I shall only say that they led to the conclusion, as far as the events of different nights could rightly be compared, that the higher shelter had the same efficacy with the lower in preventing the occurrence of cold upon the ground in a clear night, greater than that of the atmosphere, provided the oblique aspect of the sky was equally excluded from the spots on which my thermometers were laid.

On the other hand, a difference in temperature, of some magnitude, was always observed on still and serene nights between bodies sheltered from the sky by substances touching them, and similar bodies which were sheltered by a substance a little above them. I found, for example, upon one night, that the warmth of grass, sheltered by a cambric handkerchief raised a few inches in the air, was 3° greater than that of a neighbouring piece of grass, which was sheltered by a similar handkerchief actually in contact with it. On another night the difference between the temperatures of two portions of grass, shielded in the same manner as the two above mentioned from the influence of the sky, was 4°. Possibly, experience has long ago taught gardeners the superior advantage of defending tender vegetables from the cold of clear and calm nights by means of substances not directly touching them, though I do not recollect ever having seen any contrivance for keeping mats, or such like bodies, at a distance from the plants which they were meant to protect. [39]

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[39] "Anything which obstructs the free aspect of the sky arrests, in proportion, the progress of refrigeration, and the slightest covering of cloth or matting annihilates it altogether. Trees trained upon a wall or paling, or

Walls, I believe, as far as warmth is concerned, are regarded as useful during a cold night to the plants which touch them or are near to them, only in two ways; first, by the mechanical shelter which they afford against cold winds, and secondly, by giving out the heat which they had acquired during the day. It appearing to me, however, that on clear and calm nights, those on which plants frequently receive much injury from cold, walls must be beneficial in a third way, namely, by preventing, in part, the loss of heat which they would sustain from radiation if they were fully exposed to the sky, the following experiment was made for the purpose of determining the justness of this opinion.

A cambric handkerchief having been placed, by means of two upright sticks, perpendicularly to a grassplat, and at right angles to the course of the air, a thermometer was laid

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plants sown under their protection, are at once cut off from a large portion of this evil; and are still further protected if within a moderate distance of another opposing screen. * * * *

"Experience has taught gardeners the advantages of warding off the effects of frost from tender vegetables, by loose straw or other litter; but the system of matting does not appear to be carried to that extent which its simplicity and efficacy would suggest. Neither does the manner of fixing the screen exhibit a proper acquaintance with the principle upon which it is resorted to:—it is generally bound tight round the tree which it is required to protect, or nailed in close contact with its foliage.

"Now it should be borne in mind that the radiation is only transferred from the tree to the mat, and the cold of the latter will be conducted to the former in every point where it touches. Contact should, therefore, be prevented by hoops or other means properly applied, and the stratum of air which is enclosed will, by its low conducting power, effectually secure the plant. With their foliage thus protected, and their roots well covered with litter, many evergreens might doubtless be brought to survive the rigour of our winters, which are now confined to the stunted growth of the greenhouse and conservatory." Daniell:—*Climate considered with regard to Horticulture.*

"Various experiments were made to ascertain the effect of covering plants at night by matting, or other thin substances; and it was always found that when the protecting substances touched the plant much heat was conducted away from it, and such plant was at a lower temperature than when the substance was merely interposed between it and the sky; the thinnest substance upon the grass close to the lower edge of the handkerchief on its windward side. The thermometer thus situated was several nights compared with another lying on the same grassplat, but on a part of it fully exposed to the sky. On two of these nights, the air being clear and calm, the grass close to the handkerchief was found to be 4° warmer than the fully exposed grass. On a third the difference was 6°. An analogous fact is mentioned by Gersten, who says that a horizontal surface is more abundantly dewed than one which is perpendicular to the ground.

IV. The covering of snow, which countries in high latitudes enjoy during the winter, has been very commonly thought to be beneficial to vegetable substances on the surface of the earth, as far as their temperature is concerned, solely by protecting them from the cold of the atmosphere. But were this supposition just, the advantage of the covering would be greatly circumscribed, since the upper parts of trees and of tall shrubs are still exposed to the influence of the air. Another reason, however, is furnished for its usefulness, by what has been said in this Essay; which is, that it prevents the occurrence of the cold, which bodies on the earth acquire, in addition to that of the atmosphere, by the radiation of their heat to the heavens during still and clear nights. The cause, indeed, of this additional cold does not constantly operate,

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thus interposed, at any distance from the plant, was found to be effectual in preventing the loss of its heat by radiation. It was found, however, that when a plant was thus itself protected, but yet was exposed to any body which was exposed to the sky, more heat radiated from the former to the latter, than from the exposed body to the plant, and thus it lost some heat." Glaisher:—*Phil. Trans.* 1847.

"In the clear blue sky of the Valley of Chamouni, if the crops should not have ripened towards the end of the season, the peasants make fires of green wood on the two sides of the enclosing mountain, the smoke of which, uniting in the middle, forms a kind of cloudy canopy, which not only prevents the escape of radiant heat but increases its intensity, and prevents the formation of frost."—*Quarterly Review,* Vol. xxii.

but its presence during only a few hours might effectually destroy plants which now pass unhurt through the winter. Again, as things are, while low vegetable productions are prevented, by their covering of snow, from becoming colder than the atmosphere in consequence of their own radiation, the parts of trees and tall shrubs which rise above the snow are little affected by cold from this cause. For their outermost twigs, now that they are destitute of leaves, are much smaller than the thermometers suspended by me in the air, which in this situation very seldom became more than 2° colder than the atmosphere. The larger branches too, which if fully exposed to the sky, would become colder than the extreme parts, are, in a great degree, sheltered by them; and, in the last place, the trunks are sheltered both by the smaller and the larger parts, not to mention that the trunks must derive heat by conduction through the roots, from the earth kept warm by the snow.*

In a similar way is partly to be explained the manner in which a layer of earth or straw preserves vegetable matters in our own fields from the injurious effects of cold in winter.

V. The bare mention of the subject of this article will be apt to excite ridicule, it being an attempt to show in what way the exposure of animal substances to the moon's light promotes their putrefaction. I have no certain knowledge that such an opinion prevails anywhere at present, except in the West Indies; but I conclude, from various circumstances, that it exists also in Africa, and that it was carried thence by negro slaves to America. It was entertained, however, by persons of considerable rank and intelligence among the

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* It may be remarked here, however, that a thick covering of snow, while it renders the surface of the earth warmer than it would otherwise be, must occasion the lower atmosphere to be colder, by preventing the passage of the heat of the ground to the air, either by radiation or conduction.

ancients; for Pliny* affirms it to be true, and Plutarch, after making it a subject of discussion in one of his Symposia,† admits it to be well founded.

As moonbeams communicate no sensible heat to the bodies on which they fall it seems impossible that they can directly promote putrefaction. But still a reason for ascribing such a power to them may be derived from their being received by animal substances, at the very time that a real, but generally unnoticed, cause of putrefaction in warm climates (and it is in these alone the opinion I am treating of has ever prevailed) is taking place, which ceases to act as soon as the moon's light is excluded.

The nights, on which a steady moonshine occurs, must necessarily be clear; and nights which are clear are almost always calm.‡ A moonshiny night, therefore, is one on which dew forms plentifully; hence the expressions 'roscida' and 'rorifera luna' employed by Virgil and Statius; and hence also an opinion, held, as appears from Plutarch, even by philosophers among the ancients, that the moon communicates moisture to the bodies which are exposed to its light.§

Animal substances are among those which acquire dew in the greatest quantity. To do this, indeed, they must previously become colder than the atmosphere; but, having acquired the moisture of dew, in addition to their own, they will, on the following day, be in that condition, which is

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* Lib. ii. §. civ. † Lib. iii. Prob. x.

‡ M. De Luc has remarked that clouds frequently disappear soon after sunset. *Idées sur la Meteorologie,* II. 98. I have often observed this myself, and at the same time another fact of which he takes no notice; namely, that the atmosphere is then calmer than it had been before sunset. This calmness of the air very commonly, if not always, precedes the dissipation of the clouds.

§ Akin to this opinion of the ancients respecting the humefying quality of the moon, is one which has been held by modern writers as well as ancient, upon that planet's being a cause of cold to the bodies which receive its rays; though I know of no author who has taken notice of this affinity.

known, by experience, to favour putrefaction most powerfully in hot climates.

The immediate cause assigned here for the quick putrefaction of animal substances which have been exposed to the moon's rays in a hot country, is the same as that given by Pliny and Plutarch; but they attributed the origin of this immediate cause, the additional moisture, to the peculiar humefying quality which they supposed that luminary to possess. This false theory has, probably, contributed to discredit, with the moderns, the circumstance which it was employed to explain.

VI. The last fact, of which I shall treat in this Essay, is the formation of ice during the night in Bengal, while the temperature of the air is above 32 .

I have seen only two original descriptions of this process, both of which are contained in the Philosophical Transactions ; the first, by Sir Robert Barker, in the 65th volume; the other in the 83rd, by Mr. Williams.

According to the method followed by Sir R. Barker's icemaker, square excavations, 2 feet deep and 30 wide, having been formed in a large open plain, their bottoms are covered with sugar-cane, or stems of Indian corn, dried, to the thickness of 8 inches or 1 foot. On this layer are afterwards placed, in rows, near to each other, *small*, unglazed earthen pans, a quarter of an inch thick, and 1 inch and quarter deep, filled with *boiled soft* water. The pans are sufficiently porous to allow their outer surface to appear moist after water has been poured into them. Sir R. Barker adds; that the nights, the most favourable for the production of ice, are those which are the calmest and most serene, and on which very little dew appears after midnight; that clouds and frequent changes of wind are certain preventives of its formation; and that, although ice is thus very readily procured by art in

Bengal during the winter, it scarcely ever occurs there naturally.

The process described by Mr. Williams must, from its extent, 300 persons being employed in it, have been carried on for profit, and would, consequently, be conducted in the most economical manner. A piece of ground, nearly level, containing about 4 acres, was divided into square plats, from 4 to 5 feet wide, which were surrounded by little mounds of earth, 4 inches high. In these inclosures, previously filled with dry straw, or sugar-cane haum, were placed as many *broad*, shallow, unglazed earthen pans, containing *unboiled pump* water as they could hold. The air was generally very still, when much ice was formed; wind prevented its formation altogether. In the morning, between 5 and 6h., at which time alone Mr. Williams made his observations, a thermometer, with its bulb naked, placed on the straw amidst the freezing vessels, was never found by him lower than 35°; and he has observed ice, when a thermometer so placed was 42°. Another thermometer, suspended 5½ feet above the ground, was *commonly* 4° higher than that among the pans. It is possible, therefore, that Mr. Williams may have seen ice a little before sunrise, when the temperature of the air was 46°. But granting this were the fact, it would not hence follow that the ice was formed while the air possessed that heat. For, although the air is generally held to be in all countries colder about sunrise than at any other time, I know from my own observations, that this is not *always* the case in England; and similar exceptions may occur in Bengal. Sir H. Davy has said, in his Elements of Chemistry, that ice will form in Bengal when the temperature of the air is not below 50°; but he has given no authority for this assertion.

The formation of ice in the circumstances which have been just mentioned, was attributed by Sir R. Barker altogether, and by Mr. Williams in great measure, to cold produced by evaporation. Sir R. Barker's opinion has since been adopted by some of our most distinguished writers on Natural Philosophy, as Watson, Thompson, Young, Davy, and Leslie, apparently, however, without their having fully considered it, as I shall now attempt to show.

1. It is necessary for the complete success of the process that the air should be very still; wind, which so greatly promotes evaporation, prevents the freezing altogether. Sir R. Barker admits that the excavations in the earth are made to increase the stillness of the air in contact with the water in the pans; but, with the view to explain the utility of this stillness he supposes, in opposition to all experience, that water kept very quiet freezes more readily, when other circumstances are the same, than if it were a little agitated.

2. No proof is given that evaporation from the pans actually does occur at the times which are the most favourable for the appearance of ice. At any rate it cannot be considerable, since, agreeably to what is mentioned by Sir R. Barker, dew forms in a greater or less degree during the whole of the nights the most productive of ice; and it is not to be thought, as was said upon a former occasion, that one portion of air will be depositing moisture, from possessing a superabundance of it, while another in the immediate vicinity is receiving moisture in great quantity in the state of pellucid vapour, as the latter fact can exist only when the air is far removed from a state of repletion with water.

3. If evaporation produced the cold under consideration, the wetting of the straw or other matter upon which the pans are placed would tend to increase it; and, accordingly, Sir H. Davy affirms this to be the case. But Mr. Williams, who must here be regarded as the better authority, says that it is *necessary* to the success of the process that the straw be dry; in proof of which he mentions that when the straw becomes wet by accident it is replaced; and that when he

purposely wetted it in some of the inclosures the formation of ice there was always prevented. The reasons are clear. The water, by softening the straw, renders it easily compressible by the weight of the pans, and at the same time fills up what would otherwise be vacant spaces among its parts. The straw, therefore, in this condensed state, must afford a ready passage to heat from the earth to the pans, the hindrance of which is allowed by every person to be the use of it in this process when dry. Again; the moisture which passes through the straw to the earth it covers will rise afterwards in the form of vapour, having the same temperature with the warm ground, and will communicate heat to the pans. In the last place; a part of this vapour will be condensed into water by the pans, in consequence of which heat must be extricated.

4. It is mentioned both by Sir R. Barker and Mr. Williams, in support of their opinions, that the pans, when new, are so porous that they readily permit water to transude them; and that old pans, which permit this in a less degree, are less fit for the making of ice. But the argument, which is hence derived by them, is completely refuted by a fact related by Mr. Williams himself, for he says that the pans are greased before they are used, to prevent the adhesion of the ice to their sides, since, if this purpose be answered, the water can never be in contact with the pans, and therefore can never pass through them.

The real reason of the less fitness of old pans for the making of ice is perhaps the following: The production of the cold which occurs in this process must take place in the water; since neither the straw upon which the pans are placed, nor the air above them, was ever found by Mr. Williams of so low a temperature as 32°. Whatever, therefore, obstructs the passage of heat from the straw to the water must favour the freezing of the latter. But this will be less effectually done by an old than by a new pan, as the density of the former is greater, from the grease forced into it by rubbing, and from the slime and sand that will enter with the water into its pores, when these are not entirely closed by the grease; which must often happen, as the smearing is performed only once in three or four days. The difference, however, in effect betwixt old and new pans must be very small, as it does not appear that the old are ever laid aside on account of their unfitness.

In a like way may be explained, without the aid of cold produced by the evaporation of moisture from the outsides of the pans, another fact mentioned by Mr. Williams, that ice was often found by him in those vessels, while water contained in a china plate surrounded by them, had none; since the thin and dense substance of the plate must have transmitted more readily than the thick and rare substance of the pans, the heat of the straw to the water.

5. In accounting for the making of ice in Bengal, it is requisite to show, not only how the first film is produced, but also in what way the thickness of this film is afterwards increased. If evaporation be the cause of this increase, it follows, that a plate of ice in the night-time, and in the stillest air, both unfavourable to that process, must yet emit as much moisture as is necessary for the production of a cold, according to Mr. Williams, of at least 14°, and according to Sir H. Davy, of at least 18°, a conclusion, as it appears to me, of itself sufficient to destroy the credit of the theory from which it is drawn.

While attending to this subject, I became desirous of acquiring some knowledge of the degree of cold which might be produced by evaporation from water contained in a shallow vessel. With this view I placed on a featherbed, situated between the door and window of a room in my house in London, two china plates, into one of which as much water was poured, as covered its bottom to the depth of a quarter of an inch. The other plate was kept dry. The bulb of a small thermometer being then applied to the inside of the bottom of each plate, I observed upon many days, in various seasons of the year, the difference between these instruments while the door and window were open. I found, in consequence, that when the temperature of the air in the room was 75°, the highest at which any experiment was made, the thermometer in the plate containing water, was between 6 and 7 degrees lower than the one in the dry plate; that the difference between these thermometers diminished gradually as the air became colder; and that when the temperature of the air was 40°, the lowest for which I have any observation, the difference was only 1°½. At 32°, therefore, it would have been very small, and at a few degrees below 32 it would probably have vanished. This supposition agrees with an observation made by Mr. Wilson of Glasgow, who found that no cold was produced by evaporation from snow possessing a temperature of 27°, though the air in the immediate neighbourhood was purposely much agitated by him.[40]

The conclusions here given by me respecting the cold produced by the evaporation of water, were drawn from experiments made in the day, while the sky was clear, the air very calm, and the temperature of the atmosphere stationary. At night, and during a cloudy day, the differences were less. On the other hand, if there was any perceptible motion in the air, they were greater. They were also greater if the heat of the atmosphere was increasing, but less, if this was decreasing.

Having thus, I think, placed beyond doubt that the formation of ice in Bengal is not occasioned by evaporation,

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[40] The reader may wonder at the rudeness of these experiments; but the wet-bulb thermometer was not yet thought of.

I shall now state several reasons which have induced me to believe that it depends upon the radiation of heat to the heavens.

1. This cause not only exists, but exists in a degree sufficient for the production of the effect which I attribute to it. For Mr. Wilson found the surface of snow during a clear and calm night to be 16° colder than air 2 feet above it, the temperature of the latter being taken by a naked thermometer; whereas the greatest heat of the atmosphere ever observed by Mr. Williams, at the distance of 5½ feet from the ground, during the time that he supposed ice to be forming, was only 14° higher than the freezing point of water. I need say nothing of the difference of 18° related by Sir H. Davy, as he does not speak from his own observation, and as he gives no authority for what he advances; though even this difference is considerably less than what I have attempted to show must sometimes occur, from the radiation of heat at night, between the temperature of air a few feet above the earth, and that of bodies placed on its surface.

It is to be mentioned here also, that, according to Mr. Leslie*, the power of water to radiate heat exceeds, perhaps, that of all other substances.

2. Ice is chiefly formed in Bengal during the clearest and calmest nights; and it is on such nights that the greatest cold from radiation is observed on the surface of the earth. In Sir R. Barker's more refined mode of conducting the process, an unusual stillness of the air, in contact with the water to be frozen, is procured by placing the pans containing it a little below the level of the ground; in which situation it was formerly shown bodies must grow colder from radiation to the heavens at night than in any other.

3. The cold, by means of which ice is produced in Bengal, appears, as I think may be inferred from what is said by Sir R. Barker, in its greatest degree, like cold from radiation in

* *On Heat*, p. 80.

other substances, on those still and serene nights, during which little dew is deposited by the atmosphere.

4. Clouds and wind prevent the formation of ice in Bengal; and the same states of the atmosphere either prevent, or considerably diminish, the occurrence of cold from the radiation of heat at night by bodies on the ground. [41]

I shall close this subject by giving some account of a few attempts to procure the freezing of water at night in this country, by exposing it to air of a temperature higher than that of 32°. These were made by me in 1812, at my usual place of experiment, which was formerly stated to be not well adapted for the appearance of a great cold from radiation, and on nights not among the most favourable to such an undertaking, even of those which occur in this country. It is proper also to mention that I was then less able to conduct such experiments and to make use of them than I afterwards became from a longer attention to similar objects.

EXPERIMENT FIRST.

With a view to imitate the method of making ice described by Sir R. Barker, I had a pit dug, on the evening of the 3rd of May, in the middle of the garden so often spoken of, 4½ feet long, 3 wide, and 2 deep. It consequently had the same depth as the excavations mentioned by that gentleman,

[41] "This explanation of Wells, is, no doubt, the true one. I think, however, it needs supplementing. It appears, from the description, that the condition most suitable for the formation of ice, is not only a clear air, but a *dry* air. The nights, says Sir Robert Barker, most favourable for the production of ice, are those which are clearest and most serene, and *in which very little dew appears after midnight.* I have italicised a very significant phrase. To produce the ice in abundance, the atmosphere must not only be clear, but it must be comparatively free from aqueous vapour. When the straw in which the pans were laid became wet, it was always changed for dry straw; and the reason Wells assigned for this was, that the straw by being wetted was rendered more compact and efficient as a conductor. This may have been the case, but it is also certain that the vapour rising from the wet straw and overspreading the pans like a screen, would check the chill and retard the congelation."—Tyndall *On Heat.*

but was considerably less in its other dimensions. Clean dry straw was then strewed, to the height of a foot, over the bottom of the pit. On the straw were next laid a number of small shallow earthen pans, a part of which were glazed and a part unglazed. In the last place, all the pans were filled with soft water, which had been boiled on the same evening. Contrary to my expectation, the unglazed pans remained as dry on the outside, after water had been poured into them, as those which were glazed. I conclude, therefore, that the former were more dense in their substance than the unglazed pans used in India, and that their density was probably the reason why ice did not afterwards form in them sooner than in the glazed pans which were employed by me.

Two pans, containing boiled water, were set upon the grassplat at a little distance from the pit. A watch-glass filled with boiled water was also placed upon the grassplat, and another was laid upon the raised board, which had been thinly covered with sand. All these arrangements were not completed before 10 h. at night.

At 1 h. in the morning, ice appeared in the watch-glasses on the grassplat and raised board, the heat of the air, as measured by a naked thermometer, being then, at 4 feet above the ground, $39°\frac{1}{3}$, and at 7 feet, $40°\frac{1}{2}$. At 2 h. ice was observed in the pans in the pit, while a thermometer in the air, $2\frac{1}{2}$ feet above the ground, was $36°\frac{1}{2}$. Shortly afterwards ice began also to form in the pans upon the grassplat. The temperature of grass fully exposed to the sky was at the same time 30°, while that of the earth an inch below the bottom of the grass was 45°. During the time of these observations dew formed copiously.

EXPERIMENT SECOND.

My next attempt was in the manner mentioned by Mr. Williams.

On the evening of the 22nd of May I encompassed a square piece of level ground, the sides of which were 3 feet long, with a border of earth 4 inches high, and filled the area with dry straw. On this were placed several of the earthen pans which had been formerly used, and a few smaller vessels, all containing unboiled water. After an exposure of little more than an hour, water in a watch-glass upon the straw was found frozen, the temperature of the air 2 feet above the straw being then 37°. In half an hour more ice began to appear in the earthen pans, while a thermometer 5½ feet above them, this being the height at which Mr. Williams used to suspend his instrument, was 36°. The air soon after became colder, but its temperature was never less than 33°, though taken by a naked thermometer, which, as was before said, upon a clear and calm night, occasions the air to seem about 2° colder than it really is.

It might be inferred, from what is mentioned by Mr. Williams, that the temperature of the straw beds, on which the ice pans were set at Benares, was always found by him above the freezing point, for this reason, that the straw, from containing no moisture, could not, like the water, grow cold by evaporation. I had, therefore, been surprised during the first experiment, for I had then but little acquaintance with the phenomena of cold observed with dew, that a thermometer, laid upon an exposed part of the straw, was always below the freezing point after ice had begun to form in the pans. On reading, however, his account of the process a second time with increased attention, my wonder ceased. For, as the pans he speaks of were *large*, and touched one another, and as all the pans employed in India for the making of ice widen as they rise from the bottom, like our milk pans, the thermometer, placed by him on the straw, must have been secluded from all view of the sky, and would therefore mark a temperature much higher than if it had been laid, as in my experiment, upon straw fully exposed to the heavens. On this, the second night, therefore, I placed a thermometer under the edge of one of the pans lying on the straw bed, and found it some time afterwards 6° higher than a similar instrument upon a part of the straw bed which was uncovered. Generally, however, the difference was not so great. If my pans had been large, like those of Mr. Williams, I should, no doubt, have observed more considerable differences; for, in consequence of their smallness, I could not lay a thermometer on the straw bed so as to be fully screened from the sky by the edge of any of them, without its being almost in contact with the vessel, every part of which was always colder than the sheltered straw.

Much dew formed in the course of this night. The greatest difference remarked by me during it, between the temperatures of grass and of air, was 6°, and between those of air and a fully exposed part of the straw bed 9°.

EXPERIMENT THIRD.

This was begun on the evening of the 16th of October, and was likewise made agreeably to the method related by Mr. Williams.

Ice appeared in the pans when the temperature of the air, at the height of 5½ feet, was, according to a naked thermometer, 37°.

On this night I placed upon the straw bed a dry earthen pan among those which contained water, and found the inside of its bottom to be as much colder than the air as the water was in the other pans before ice appeared in them. After the water had begun to freeze, no proper comparison could be made between its temperature and that of the empty pan. This pan in the course of the night attracted moisture, which was afterwards converted into a film of ice.

But the chief fact established by the present experiment was, that water may freeze at night in air of a temperature higher than 32°, not only without any loss of weight from evaporation, but with a gain of weight from an opposite process.

I had observed that water, exposed early in the evening in the open air to the sky, lost a little weight in the course of a clear night. This I imputed to evaporation taking place before the water had been cooled enough to condense the vapour of the atmosphere, and to the weight gained afterwards being insufficient to compensate the previous loss. I exposed, therefore, on this night, water to the influence of the sky until it was cooled to 34°. Of this I put 2 ounces into each of two china saucers, which had also been exposed to the air, and then placed the saucers upon the straw bed. In the morning a thin cake of ice was found in both saucers, one of which had gained 2½ and the other 3 grains in weight.

Dew was also copious on this night. At one time grass was 9°½, and the exposed part of the straw bed 12° colder than the air.*

It must be evident to every person that the formation of ice, in the three preceding experiments, was the effect of a natural operation, similar to that by which the same substance is produced in Bengal. These two facts must, therefore, have a common cause, and this has been shown by the last experiment, independently of what was said before in this Essay, not to be evaporation. It is also clear that the cold, induced on the water in those experiments, had a common cause with that observed at the same time upon the grass and the straw, which latter cold must, in consequence of proofs formerly given, be admitted to have arisen from the radiation of the heat of those substances to the heavens. A necessary inference, therefore, appears to be, that the formation of ice in Bengal, in the circumstances described by Sir

* The greater cold observed in this and the preceding experiment upon straw than upon grass, is to be referred to the shortness of the latter, by reason of which heat was readily communicated to its upper parts by the earth.

R. Barker and Mr. Williams, must likewise be attributed in by far the greater measure, if not altogether, to a loss of heat, which the water suffers by its own radiation while situated in such a manner that it can receive little heat from other bodies, either by radiation or conduction.*

CONCLUSION.

The experiments which were made by me on dew, and other subjects treated of in the preceding Essay, were unavoidably attended with many inconveniences, which were the more felt as my health had long been feeble, and as my professional duties obliged me often to return to London in the morning, without having previously taken rest after the whole of a night had been spent in attending to the objects of my pursuit. The inconveniences here alluded to were, indeed, so great, that I was twice or thrice obliged to intermit my labours for several months together, and at length found it necessary to cease from them entirely, before I had nearly completed the plan which I had formed. I take the liberty of mentioning these things to excuse, in part, the imperfec-

* On the evenings preceding the nights, during which ice is produced in Bengal, the temperature of the water exposed in the pans is, probably, often 60° or more. But water of the heat of 60°, if exposed in a shallow earthen vessel to air of the same temperature during the day, while the weather is calm and clear, will lose about 3° of heat by evaporation. A cold from this cause may, therefore, concur with that from radiation, and, consequently, may, in Bengal, accelerate somewhat the formation of ice. The influence, however, of evaporation there, in this respect, should the state of the air with regard to moisture still permit it, which must often not be the case while dew is forming, will, as the night proceeds, gradually diminish, and at length almost disappear before the freezing of the water commences; since I have lately shown that evaporation from water of 32° produces very little cold, even in the day time. Indeed, it seems to me much more probable, that on a clear and calm night, though in a dry winter of Bengal, water at the temperature of 32° will acquire warmth from the formation of dew upon it than that it will become cold from evaporation.

tions which will be observed in what I have written, as some of them would, no doubt, have been removed by a further interrogation of Nature.*

London, September 25, 1815.

* Of the experiments related in the beginning of the second Part of this Essay, with the view of proving, that the formation of dew is an effect of previous cold in the substances on which it appears, those of only one evening were remarkable for the greatness of their results, the weather upon the other evenings not having favoured much my purpose. I took advantage, therefore, of being in the country, at the distance of a few miles from London, on the 21st of the present month, the last day but one of an unusually long tract of dry weather, to expose to the sky, 28 minutes before sunset, weighed parcels of wool and swansdown, upon a smooth, unpainted, and perfectly dry fir table, 5 feet long, 3 broad, and nearly 3 in height, which had been placed an hour before in the sunshine in a large level grass field. At this time, and throughout my experiments, the air was very still, and the sky very serene. The atmosphere, too, in all probability, contained but little moisture, in consequence of the long absence of rain; and the surface of the ground apparently contained none. The wool, 12 minutes after sunset, was found to be 14° colder than the air, the temperature of the latter being measured by a naked thermometer suspended 4 feet above the ground, and to have acquired no weight. The swansdown, the quantity of which was much greater than that of the wool, was at the same time 13° colder than the air, and was also without any additional weight. In 20 minutes more the swansdown was 14°½ colder than the neighbouring air, and was still without any increase of its weight. My experiments now ceased from a failure of daylight.

In my former experiments of this kind, the greatest cold observed by me from radiation, without the appearance of dew, was only 9°½.

While making the experiments on wool and swansdown, I attended frequently to the temperature of the grass, and found it at one time 15° colder than that of the air 4 feet above the ground. This difference is 1° greater than any I had ever before seen between the temperatures of the same substances, and is equal to the greatest which I had ever known to occur between those of the atmosphere and of swansdown lying upon grass. I had this evening placed no swansdown upon grass.

These experiments were not made till nearly the whole of the present edition of my Essay was printed, and could not, therefore, be mentioned in their proper place.

APPENDIX.

APPENDIX.

THE ACTION OF AQUEOUS VAPOUR UPON RADIANT HEAT.

If the doctrine propounded by Professor John Tyndall, F.R.S., regarding the aqueous vapour of the atmosphere in relation to solar and terrestrial radiation, receives confirmation from the experiments of other investigators, considerable modification will be necessary in the received explanations of various meteorological phenomena, and of dew among them. In order to present the reader with the most complete information respecting the subject of dew, it is desirable to throw into an Appendix matter which would have been inappropriate as notes, but without a knowledge of which the Essay on Dew could not be studied to full advantage at the present day. For the greater portion of this article we are indebted to the writings of Professor Tyndall, and we here publicly acknowledge and thank him for his courteous assent to our request to be permitted to quote them. The reader will please to remember that quotations not otherwise acknowledged are from Professor Tyndall's pen.

"Aqueous vapour is always diffused through the atmosphere. The clearest day is not exempt from it; indeed, in the Alps, the purest skies are often the most treacherous, the blue deepening with the amount of aqueous vapour in the air. Aqueous vapour is not visible; it is not fog; it is not cloud; it is not mist of any kind. These are formed of vapour which has been condensed to water; but the true vapour is an impalpable transparent gas. It is diffused everywhere throughout the atmosphere, though in very different proportions."

"The quantity of this vapour is small. Oxygen and nitrogen constitute about 99½ per cent. of our atmosphere; of the remaining 0·5, about 0·45 is aqueous vapour, the rest is carbonic acid."

Professor Tyndall has experimentally demonstrated: that aqueous vapour is a powerful absorbent of radiant heat; that dry air is perfectly transparent, or acts as a vacuum, to radiant heat; that the more humid the air the more radiant heat it absorbs; that, for the same per centage of moisture, the rate of absorption varies as the pressure.

"Compared atom for atom, the absorption of an atom of aqueous vapour is 1600 times that of air. Now, the power to absorb and the power to radiate are perfectly reciprocal and proportional. The atom of aqueous vapour will therefore radiate with 1600 times the energy of an atom of air."

"Regarding the earth as a source of heat, no doubt *at least* 10 *per cent. of its heat is intercepted within ten feet of the surface.* This single fact suggests the enormous influence which this newly developed property of aqueous vapour must have in the phenomena of meteorology."

The sun pours its heat upon the surface of the earth during the day; and the earth at night emits its acquired heat into space. When the day is longer than the night the earth has a balance in its favour, which goes on accumulating during the summer season; but a proportionate daily loss by emission occurring in winter, the annual temperature of the earth remains constant. Such is the grand result; but highly important functions are exercised upon these processes by the variable conditions and amounts of aqueous vapour present in the atmosphere from day to day.

Fog or mist causes a sensation of cold, and interferes with the normal healthy action of the skin and the lungs. The cause is now apparent in its property of absorbing heat from the living body.

The action of aqueous vapour in arresting the emission of heat into space renders it a kind of blanket to the surface of the earth, and its presence must always tend to maintain a medium temperature equally warding off the fierce heat of the day, and maintaining a higher temperature at night. Thus vapour—whether transparent and invisible, or visible as clouds, fog, or mist—is intimately connected with the important operations of solar and terrestrial radiation. Cloudy or humid days diminish the effect upon the soil of solar radiation; similar nights retard the radiation from the earth. A dry atmosphere must be the most favourable for the direct transmission of the sun's rays; while equally the withdrawal of the sun from any region over which the air is dry must be followed by very rapid cooling of the soil.

The formation of dew, attendant upon nocturnal radiation, Professor Tyndall explains as follows:—

"The aqueous vapour of our atmosphere is a powerful radiant, but it is diffused through air which usually exceeds its own mass more than one hundred times. Not only, then, it own heat, but the heat of the large quantity of air which surrounds it must be discharged by the vapour, before it can sink to its point of condensation. The retardation of chilling due to this cause enables good solid radiators, at the earth's surface, to outstrip the vapour in their speed of refrigeration; and hence upon these bodies aqueous vapour may be condensed to liquid, or even congealed to hoar-frost, while at a few feet above the surface it still maintains its gaseous state."

The amount of condensation will depend upon the existing atmospheric conditions. In an overcast state of the sky the radiation from the earth and the counter-radiation from the clouds will be equal, or nearly so; there will not be sufficient cooling for the formation of dew. In clear air with transparent vapour present, and therefore comparatively small in amount, the counter-radiation will be less, the earth will cool, and dew may be formed. In the clearest and driest state of atmosphere, *vapour never being absent altogether in the lowest stratum,* counter-radiation will be minimised, the earth's surface will cool rapidly, the deposit of dew will be copious; a gentle wind being favourable for the full effect.

"The difference between a thermometer, which, properly confined, gives the true temperature of the night air, and one which is permitted to radiate freely towards space, must be greater at high elevations than at low ones;" because, there being a less thickness of vapour present over high elevations, the passage of radiant heat from the earth will be less impeded.

"The following remarkable passage from Hooker's *Himalayan Journals,* 1st edit. vol. II., p. 407, bears upon the present subject: 'From a multitude of desultory observations I conclude that, at 7,400 feet, 125°.7, or 67° above the temperature of the air, is the average effect of the sun's rays on a black bulb thermometer. . . . . . These results, though greatly over those obtained at Calcutta, are not much, if at all, above what may be observed on the plains of India. The effect is much increased by elevation. At 10,000 feet, in December, at 9 a.m., I saw the mercury mount to 132°, while the temperature of shaded snow hard by was 22°. At 13,000 feet, in January, at 9 a.m., it has stood at 98°, with a difference of 68°·2, and at 10 a.m. at 114°, with a difference of 81°·4, *whilst the radiating thermometer on the snow had fallen at sunrise to* 0°·7.'

"These enormous differences between the shaded and unshaded air, and between the air and the snow, are, no doubt, due to the comparative absence of aqueous vapour at these elevations. The air is incompetent to check either the solar or the terrestrial radiation, and hence the maximum heat in the sun and the maximum cold in the shade must stand very wide apart. The difference between Calcutta and the plains of India is accounted for in the same way."

Mons. C. Martins found on the summit of the Pic du Midi the heat of the soil exposed to the sun to be twice as great as in the valley at the base of the mountain; and he remarks, "The immense heating of the soil, compared with that of the air on high mountains, is the more remarkable, since, during the nights, the cooling by radiation is there much greater than in the plain."

"Wherever the air is dry we are liable to daily extremes of temperature. By day, in such places, the sun's heat reaches the earth unimpeded and renders the maximum high; by night, on the other hand, the earth's heat escapes unhindered into space and renders the minimum low. Hence the difference between the maximum and minimum is greatest where the air is driest."

When air is rarefied, a partial condensation of its vapour takes place. Aqueous vapour, ascending into the atmosphere, gradually attains regions more and more rarefied, and the accompanying chilling must have a certain condensing effect. The surface of the vapour presented to the clear firmament will likewise impart its heat to space by radiation; and must, as a secondary cause, produce condensation. The like result will accompany the deflection upwards of moist winds by mountains.

"Cumuli are the heads of columns of vapour, which rise from the earth's surface, and are precipitated as soon as they reach a certain elevation. Thus, the visible cloud forms the capital of an invisible pillar of saturated air. Certainly the top of such a column, raised above the lower vapour-screen which clasps the earth, and offering itself to space, must be chilled by radiation; in this action alone we have a physical cause for the generation of clouds."

"A freedom of escape, similar to that from bodies of vapour at great elevations, would occur at the earth's surface generally, were the aqueous vapour removed from the air above it; for the body of the atmosphere is a perfect vacuum as regards the transmission of radiant heat. The withdrawal of the sun from any region over which the atmosphere is dry, must be followed by quick refrigeration. . . . . . The winters of Thibet are almost unendurable from this cause. . . . The removal, for a single summer night, of the aqueous vapour from the atmosphere which covers England, would be attended by the destruction of every plant which a freezing temperature could kill. In Sahara, 'where the soil is fire and the wind is flame,' the refrigeration at night is often painful to bear. Ice has been formed in this region at night. In Australia, also, the *diurnal range* of temperature is very great, amounting, commonly, to between 40 and 50 degrees. In short, it may be safely predicted, that wherever the air is *dry*, the daily thermometric range will be great. This, however, is quite different from saying that where the air is *clear*, the thermometer range will be great. Great clearness to light is perfectly compatible with great opacity to heat; the atmosphere may be charged with aqueous vapour while a deep blue sky is overhead, and on such occasions the terrestrial radiation would, notwithstanding the clearness, be intercepted."

The important discovery of Dr. Tyndall, that the deportment of aqueous vapour and that of dry air are totally dissimilar towards radiant heat, opens out to meteorologists a fresh arena for thought and investigation. Hitherto the deportment of the atmospheric air in its aggregate condition of dry gases and diffused vapour in relation to radiation has been alone considered; and the neutral character of dry air was certainly never suspected. It would seem, therefore, that the meteorologist has in future mainly to confine his attention to aqueous vapour as the prime agent in the phenomena attending solar and terrestrial radiation, while equally the action of radiation must be kept in contemplation in investigating the phenomena in which aqueous vapour is chiefly concerned. The new doctrine will probably modify the explanations given in existing treatises and memoirs of numerous physical phenomena. For instance, how can the following statement from Herschel's *Meteorology* confront the fact that dry air is as transparent to radiant heat as a vacuum? "It is a property of caloric radiant from bodies heated below incandescence to be eminently absorbable by transparent media."

The reader has already perceived how Tyndall's new teaching modifies the received explanations of the formation of dew, of cloud, of rain, of the cause of wide ranges of temperature. In our note to Wells's Essay on Dew it will be seen that it gives an addition to the explanation of the artificial mode of obtaining ice practised in Bengal. We give yet another interesting point on which this subject has a bearing, the theory of *sérein.* "'Most authors,' writes Melloni, 'attribute to the cold, resulting from the radiation of the air, the excessively fine rain which sometimes falls in a clear sky, during the fine season, a few moments after sunset. But,' he continues, 'as no fact is yet known which directly proves the emissive power of pure and transparent elastic fluids, it appears to me more conformable,' &c. &c. If the difficulty here urged against the theory of *sérein,* be its only one, the theory will stand, for transparent elastic fluids are now proved to possess the power of radiation which the theory assumes. It is not, however, to radiation from the *air* that the chilling can be ascribed, but to radiation from the body itself, whose condensation produces the *sérein.*"

Professor Tyndall has shown that water is opaque to the radiation from aqueous vapour which, indeed, is composed of the same constituents in a different state of aggregation, and when in contact must, as in a steam boiler, preserve the same temperature. The opacity of aqueous vapour to the radiation from water may be inferred, and hence may be concluded that "the very act of nocturnal refrigeration, which causes the condensation of water on the earth's surface, gives to terrestrial radiation that particular character which renders it most liable to be intercepted by our atmosphere, and thus prevented from wasting itself in space."

"The extraordinary energy of water as a radiant, *in all its states of aggregation,* must play a powerful part in a mountain region. As vapour, it pours its heat into space, and promotes condensation; as liquid, it pours its heat into space, and promotes congelation; as snow, it pours its heat into space, and thus converts the surfaces on which it falls into more powerful condensers than they otherwise would be."

The meteorological instruments employed for measuring the intensity of both solar and terrestrial radiation, and the temperature of air and of evaporation, ought to furnish observations from which the effect on radiant heat of the aqueous vapour actually in the atmosphere could be made apparent. Attempts, indeed, have been made recently to exhibit this effect. *The Philosophical Magazine, No.* 212, contains the results, from observations made at Melbourne, bearing on the absorption of heat by aqueous vapour, and its relation to terrestrial radiation, by M. G. Neumayer. His deductions cannot be accepted *per se,* for his mode of observation was certainly open to objection. The thermometer employed to measure the radiation was placed in a parabolic reflector, that in an open box filled with cotton, raised one foot-and-a-half above the ground, and the whole placed inside a close fence, so that the zenith over the instrument was only free for the space of 38° across. The neutralising effect of partial screens has been so well demonstrated by Wells, that, confirmed as it has been by others, it is surprising that a meteorologist should expect to detect small differences by endeavouring to restrict the essential conditions for the production of the full effect. The position of the instrument was arbitrary and novel—the usual plan being to place the reflector on the soil or grass.

An altogether unobjectionable, and a more successful attempt to exhibit the action of aqueous vapour on terrestrial radiation, has been made by Colonel Strachey, F.R.S., an account of which will be found in the *Philosophical Magazine*, *No.* 213. From the Madras observations, for the years 1841-2-3-4, those days were selected on which the sky was almost free from clouds; then the fall of temperature from 6 h. 40 m. p.m., to 5 h. 40 m. a.m., and the mean tension of vapour during the night were calculated, and the temperature at 6 h. 40 m. noted. The following is a statement deduced from the Colonel's tables for each year; the mean of all the observations when the tension of vapour was between 1·00 inch and ·90, between ·89 and ·80, and so on, being taken:—

Tension of vapour.	Fall of thermometer 6 h. 40 m. p.m., to 5 h. 40 m. a.m.	Thermometer at 6 h. 40 m. p.m.	Number of observations.
INCH.	°	°	
0·93	4·9	85·8	27
·85	6·1	84·6	13
·72	7·1	80·5	23
·65	8·3	78·8	32
·58	8·7	76·8	8

"The general conclusion, that the loss of heat by radiation from the earth at night is directly affected in a very considerable degree by the quantity of vapour in the air, seems to be unmistakeably indicated by these figures. Just the same results are shown in all other cases in which I have been yet able to examine such observations of night temperatures under a clear sky; and the following figures are got from observations made between the 4th and the 25th of March, 1850 (also taken from the Madras records), during, which period the sky remained clear, and great variations of the quantity of vapour took place.

Tension of Vapour	·888	·849	·805	·749	·708	·659	·605	·554	·435
Fall of Temperature from 6 h. 40 m. P.M. to 5 h. 40 m. A.M.	6°·7	7°·1	8°·3	8°·5	10°·3	12°·6	12°·1	13°·1	16°·5

"The temperature of the air at 6 h. 40 m. p.m., varied from 78°·9 to 84°·7."

"The variation of the heating power of the sun prevents useful comparisons between the increase of heat during the day, at different times of the year. But the fact comes out clearly enough, that the air, when dry, is more freely traversed by the sun's heat than when damp. The observations of March, 1850, arranged so as to bring out this point, give the following results:—"

Tension of Vapour.	·824	·737	·670	·576	·511	·394
Rise of Temperature from 5 h. 40 m. a.m. to 1 h. 40 m. p.m.	12°·4	15°·1	19°·3	22°·2	24°·3	27°·0

Colonel Strachey's investigation tends to confirm the views entertained by Professor Tyndal.

Experiments made by Professor Magnus* lead him to assert, that aqueous vapour does not possess the great absorptive power attributed to it. "The radiation of *transparent* aqueous vapour (that is, of aqueous vapour in the

* *Vide Philosophical Magazine,* No. 214.

proper sense of the term)," he says, "is but little more than that of dry atmospheric air. Hence it follows that the *absorptive power* of air which contains, or is saturated with, *transparent* vapours, differs little from that of dry, and that it is only if air is misty (that is, contains condensed vapour) that it radiates and equally well absorbs heat."

"But I think these experiments were not needed. A well-known phenomenon, which depends on the radiation of heat, furnishes a more striking proof of the small absorptive capacity of aqueous vapour than all experiments in the laboratory. If aqueous vapour were in fact so good an absorbent, as Professor Tyndall maintains, dew could never be formed; for the aqueous vapour, which is indispensable for dew, would at the same time form a covering over the surface of the earth and prevent its radiation. But just where the atmosphere is particularly rich in water, in the tropics, is dew principally formed; and those regions would be devoid of all fertility were it not that moisture is imparted to the plants by dew. Should it be urged that the vapour absorbs heat indeed, but radiates a portion only back to the earth while the greater part goes to the higher regions of the atmosphere, this process of partial radiation would repeat itself from layer to layer, and hence the temperature in different layers must diminish with the height. This, however, is notoriously not the case in the formation of dew: the temperature merely sinks near the good radiating surface of the earth; and, a few feet about, it is not lower than over a badly-radiating place which is not covered with dew. Further, a cooling would be impossible, since all layers of the atmosphere, just as they radiate away part of their heat from the earth, send another portion back to it. If aqueous vapour possessed so great an absorptive capacity as Professor Tyndall ascribes to it, extremely little of the radiating heat could reach the clouds, since the enormous layer of vapour extending to them would entirely absorb it. It would then be inexplicable that clouds prevent dew. Since Wells's memorable investigations it is generally assumed that thermal rays reach the clouds almost undiminished, and are thence reflected back to the earth; if there were no such almost unhindered transmission through moist air the clouds at their great distance could not hinder the radiation as does a board or any other solid body, at a small distance from the earth."

"The conclusions which Professor Tyndall deduces for certain climatic phenomena remain unchanged if nebulous be substituted for actual vapour. For this it is which contributes to the maintenance of the beautiful green of the British Islands; for it moderates the burning rays of the sun, as well as prevents the great colds, which only occur with a clear sky and a copious radiation."

Professor Tyndall in "Remarks on the Paper by Professor Magnus"* meets objections by reflections suggested by Professor Magnus's description of his own experiments, and adds: "With regard to the formation of dew, the amount deposited depends on the quantity of vapour present in the air; and where that quantity is great, a small lowering of temperature will cause copious precipitation. Supposing 50, or even 70 per cent. of the terrestrial radiation to be absorbed by the aqueous vapour of the air, the uncompensated loss of the remaining 30 would still produce dew, and produce it copiously where the vapour is abundant. Attenuated as aqueous vapour is, it takes a good length of it to effect large absorption. I have already risked the opinion that at least 10 per cent. of the earth's radiation is intercepted within 10 feet of the earth's surface; but there is nothing in this opinion incompatible with the observed formation of dew. A surface circumstanced like that of the

* *Vide Philosophical Magazine, No.* 214.

earth, and capable of sending unabsorbed 80 or 90 per cent. of its emission to a distance of 10 feet from itself, must of necessity become chilled, and must, if aqueous vapour in sufficient quantity be at hand, produce precipitation.

"I should willingly leave to others the further development of this question, feeling assured that, once fairly recognised by field meteorologists, the evidence in favour of the action of aqueous vapour on solar and terrestrial radiation will soon be overwhelming."

An independent experimentalist, H. Wild, has followed precisely the method pursued by Professor Tyndall, and that by Professor Magnus. He did not succeed in obtaining any results from the latter method, which he was compelled to abandon. He concludes his memoir * with these words: "I feel bound, from my own experience, to give a decided preference to Tyndall's method, not only on account of the greater facility with which it furnishes qualitative results, but also in consequence of its greater delicacy. It is principally in consequence of this greater delicacy that, notwithstanding the negative results furnished by Magnus's method, I maintain that the greater absorptive power of moist air, as compared with dry, has been fully established by the experiments made according to Tyndall's method; and I am of opinion that meteorologists may, without hesitation, accept this new fact in their endeavours to explain phenomena, which hitherto have remained more or less enigmatical."

The observations registered from the solar radiation thermometer, and the terrestrial radiation thermometer, in connection with a record of the state of the sky, and the amount of cloud, enable us to test the action of aqueous vapour upon radiant heat. Thus, taking as an illustration,

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* *Vide Philosophical Magazine*, No. 216.

the observations made at the observatory at Adelaide, Australia, in January 1863, we get the following result:—

Sky during the day of 24 hours.	Mean difference between diurnal and nocturnal radiation.	Number of days, observations.
Clear	88°·3	15
Cloudy	80·2	10
Overcast	64·3	6

This example, taken at random from a voluminous record, is completely in accordance with all that Tyndall has written. It may be added that the solar thermometer used had a black bulb, and was enclosed in an exhausted tube, having a large ball, in the centre of which the bulb was placed. It was held in a horizontal position by two light wooden brackets, screwed on to a stout post, five feet above the ground. The terrestrial thermometer was a spirit minimum, and was placed on short grass.

For an account of the mode of experimenting on this subject, of the instruments employed, and of the method of reduction, reference must be made to Tyndall's work entitled "*Heat considered as a mode of Motion*," an exceedingly entertaining and instructive book.

When experimentalists are satisfied with the conclusiveness of their results, with the validity of their methods of procedure, and with the certainty of elimination of all disturbing agents in their operations, when, in short, they agree among themselves as to what to do and the effects resulting, then it will be advisable, if not necessary, to endeavour to bring the facts under the regime of condensed, cogent, and logical statements and arguments. At present the subject is open to attack. The original experimentalist, not foreseeing the importance of his enquiries into aqueous vapour, has not laboured to give his results coherency and conclusiveness. We can only pretend to having studied the subject attentively, but we trust that the account we have given of the new doctrine will tend to concentrate attention to it.

SOLAR RADIATION.

The influence of radiant heat in the economy of nature is still a subject about which very little is positively known, notwithstanding the researches of Rumford, Leslie, Davy, Prevost, Dulong, and Petit, Daniell, Melloni, Glaisher, Herschel and others, not forgetting Tyndall. Dr. Wells, in his "*Essay on Dew*," has indeed treated one particular branch of the subject, the relation between nocturnal radiation and the formation of dew, with the concomitant effects upon vegetation, with singular ability. The maximum heating effect of radiation, to which Wells had given no attention, Daniell made a subject of enquiry, and he attempted to bring together our knowledge of solar radiation, in an Essay which was first published in 1823, and which contains probably the best information which can be had even at the present time. Now, as then, a satisfactory answer cannot be given to the question: "What is the maximum calorific impression which plants are subject to in any latitude?" Influenced as the atmosphere is by the varying temperatures of the terrestrial substances acted upon by the solar power, it becomes an interesting branch of meteorological enquiry to examine, in all its generality, the varying conditions of this problem. The subject is replete with difficulties; there is not yet even a recognised standard instrument which will furnish absolute indications of its effects, and properly measure them. Various means have been employed for the purpose. Thermometers have been used with the bulbs naked, blackened bright, blackened dull, or covered with black wool; sometimes enclosed *in vacuo* in glass jackets, but more frequently not; placed either on the soil itself, in the *foci* of metallic reflectors, or in blackened recesses, resting on the soil and exposed to the sun's rays. Few of the rays impinge directly upon the bulb of the thermometer so placed; the reflection from it must be considerable, varying with size and shape, so that no two instruments agree probably in their indications.

"Different kinds of black glass differ notably as to their power of transmitting radiant heat. In thin plates some descriptions tint the sun with a greenish hue, others make it appear a glowing red without any trace of green. The latter are by far more diathermic than the former. In fact, carbon, when perfectly dissolved and incorporated with a good white glass, is highly transparent to the calorific rays." . "The black glass chosen for thermometers, and intended to absorb completely the solar heat, may entirely fail in this object, if the glass in which the carbon is incorporated be colourless. To render the bulb of a thermometer a perfect absorbent, the glass with which the carbon is incorporated ought, in the first instance, to be green."—Tyndall on "*Radiation,*" Rede Lecture, 1865.

"It is well known to the agriculturist and the gardener that without the direct influence of the sun, whatever may be the temperature of the air, the fruits of the earth seldom come to perfection." The direct heating power of the sun requires to be investigated with reference to the hour of the day, the season, the latitude, and the elevation above the sea. Daniell has shown that it follows the course of the sun's declination. The maximum intensity and effect occur in June, while the greatest mean temperature of the air does not take place till July. In the month of June, a plant may be so circumstanced in this country as to undergo all the changes of heat from 154° to 30.° "This arrangement, no doubt, has an important influence upon the processes of fructification in the vegetable kingdom. Agriculturists are well aware of the advantage of direct solar heat in the flowering of wheat, and other corn crops, an advantage which is never compensated by any elevation of temperature under a clouded sky."* According to the same authority, the force of the sun's direct radiation decreases in approaching the equator, and increases in advancing towards the poles. It also increases, from below, upwards. The same cause which obstructs the passage of radiant heat in the atmosphere from the sun, opposes also its transmission from the earth into space.

These results of the researches made by Daniell are sufficiently at variance with our common notions of heat and cold; and, though physical considerations will account for them, still they must serve to increase the general interest which the subject of radiant heat elicits.

From experiments made at the Cape of Good Hope, Sir John Herschel calculated the direct heating effect of a vertical sun, at the sea-level, to be capable of melting 0·00754 of an inch, per minute, from a sheet of ice exposed perpendicularly to its rays. M. Pouillet concluded, from similar experiments, that the effect is expressed by 0·00703 of an inch. A mean of these two numbers, 0·007285 in., may, therefore, be regarded as a measure of the sun's vertical heating power, at the sea-level, in a perfectly cloudless sky. The effect of the invisible vapour, always present in variable quantities in the clearest state of the weather, in intercepting the passage of heat to the earth, has not been taken into account in the experiments which gave these deter-

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* Daniell's *Meteorological Essays.*

minations. Could it be taken into calculation, different and more consistent results might doubtless be expected.

"The direct heating power of the sun's rays may be measured in two different ways—statically and dynamically. The statical method consists in equilibrating the heating power of sunshine on some body (as a blackened thermometer) with some external cooling influence which is itself measurable or which we have reason to believe invariable. It has been usual to suppose this accomplished by simply noting the degree marked by such a blackened thermometer in the sun (exposed till it ceases to rise), in excess of that marked by a similar one in the shade. This is much as if a man should measure his strength by the depth to which he could thrust a pole into the ground, in the absence of any knowledge of its sharpness, or the resistance of the soil. The cooling influences (conduction and radiation) are dependent for their effects on local and temporary circumstances too numerous and too variable to estimate. The measure itself requires to be measured. The objection is palliated, but not removed, by enclosing the thermometer in an exhausted glass tube, which eliminates conduction by cutting off the contact of air, and by other methods designed to deaden or equalize external influences. It is, however, insuperable in principle.

"The dynamical method consists in ascertaining the amount of physical change of a nature susceptible of definite measurement, effected on some object by a given sectional area of sunbeam in a given time; such, for instance, as the dilatation of a liquid, the melting of ice, or the raising of the temperature of a given quantity of water a certain number of degrees." * Herschel was the first to attempt such a measure of the sun's heating effect; and his experiments eventually led to the invention of the *actinometer*. Pouillet's

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* "*Meteorology*," by Sir J. F. W. Herschel.

pyrheliometer is an instrument for giving similar measurements.

The actinometer consists of a glass cylinder containing ammonio-sulphate of copper. One end of the cylinder is closed by a screw working in a liquid-tight collar. This screw must be made of silver, the only metal not acted upon by the liquid enclosed, and its object is to admit of a change of capacity when the liquid is too much dilated with heat. The other end is drawn out into a tube of narrow bore, upon which the dilatation of the liquid is measurable as in a thermometer. A spirit thermometer is inserted in the axis of the cylinder, and its stem passes through the axis of the adjusting screw. "This instrument being several times alternately exposed for one minute in the sun and shade, and the changes of volume in each case read off on the scale, the differences or sums of the mean changes, according as the action has been in the same or in a contrary direction, gives the dilatation produced by the sunshine alone (freed from the disturbing influences), corresponding to the actual temperature of the liquid, which being reduced by an appropriate table to give the temperature acquired, affords a measure of the effect of a given sectional area of sunbeam in heating a a definite volume of liquid. Finally the result is reduced to *actines*, or units of solar radiation, each actine denoting that amount of radiation which would suffice to melt one-millionth part of a metre from the thickness of a sheet of ice perpendicularly exposed, in one minute, supposing it wholly absorbed. * * * The portability and facility of use and reduction of this instrument, as well as the consistent results it affords, leave nothing to desire, and afford a perfect measure of solar radiation." Unfortunately it is very costly, the price being from £5 to £8. Instructions for using it and for reducing the observations would take too much space. The very few persons who may require them are recommended to consult the *"Admiralty Manual of Scientific Enquiry,"* where, in an article on meteorology, the inventor has himself given all the necessary information.

The instrument termed by Pouillet pyrheliometer may be constructed with water, mercury, or any other fluid; the preference, however, must be given to mercury as the more stable and manageable fluid. A shallow steel cylinder is filled with mercury. Into the cylinder the bulb of a thermometer is introduced, the stem of which, standing at right angles to the flat end of the cylinder, is protected by a piece of brass tubing. The opposite end of the cylinder is coated with lamp-black, and is the part to be exposed towards the sun. A collar and screw is fitted to the tubing, by means of which the instrument may be attached to a stake driven into the ground. "It is necessary that the surface which receives the sun's rays should be perpendicular to them, and this is secured by appending to the brass tube which shields the stem of the thermometer a disk of precisely the same diameter as the steel cylinder. When the shadow of the cylinder accurately covers the disk, we are sure that the rays fall, as perpendiculars, on the upturned surface of the cylinder. The observations are made in the following manner:—First, the instrument is permitted, not to receive the sun's rays, but to radiate its own heat for five minutes against an unclouded part of the firmament; the decrease of the temperature of the mercury consequent on this radiation being noted. Next, the instrument is turned towards the sun, so that the solar rays fall perpendicularly upon it for five minutes—the augmentation of temperature is now noted. Finally the instrument is turned again towards the firmament, away from the sun, and allowed to radiate for another five minutes, the sinking of the thermometer being noted as before. You might, perhaps, suppose that exposure to the sun alone would be sufficient to determine its heating power; but we must not forget, that during the whole time of exposure to the sun's action, the blackened surface of the cylinder is also radiating into space; it is not therefore a case of pure gain; the heat re-received from the sun is, in part, thus wasted, even while the experiment is going on; and to find the quantity thus lost, the first and last experiments are needed. In order to obtain the whole heating power of the sun, we must add to his observed heating power the quantity lost during the time of exposure, and this quantity is the mean of the first and last observations. Supposing the letter R to represent the augmentation of temperature by five minutes' exposure to the sun, and that t and t′ represent the reductions of temperature observed before and after, then the whole force of the sun, which we may call T, would be thus expressed:

$$T = R + \tfrac{1}{2}(t + t')$$

"The surface on which the sun's rays here fall is known; the quantity of mercury within the cylinder is also known; hence, we can express the effect of the sun's heat upon a given area, by stating that it is competent, in five minutes, to raise so much mercury, or so much water, so many degrees in temperature. Water, indeed, instead of mercury, was used in M. Pouillet's pyrheliometer."—Tyndall's *Heat considered as a Mode of Motion*, 2*nd Ed.*

The makers of this instrument have to take scrupulous care to measure exactly the interior diameter and depth of the steel cylinder. If the instruments could be made generally to a uniform pattern in every particular there is no doubt that extended employment of them, in various parts of the world and at different seasons, would yield valuable results.

"A thermometer with the bulb blackened affords the only means the traveller can compass of measuring the power of the sun's rays. It should be screened or put in a blackened box or laid on black wool." Such is the indefinite language used by the author of the "*Himalayan Journals*," a traveller than whom none have made better meteorological observations, or better use of them. The pyrheliometer appears to be an instrument of precision peculiarly suited to the requirements of the scientific traveller.

"Experiments made by Pouillet, at different altitudes, with the pyrheliometer, show that, everything else being equal, the generation of heat by the solar rays is more powerful in higher altitudes than near the surface of our globe, and that consequently a portion of these rays is absorbed in their passage through the atmosphere. Why, in spite of this partial absorption, the mean temperature of low altitudes is nevertheless higher than it is in more elevated positions, is explained by the fact that the atmosphere stops to a far greater degree the calorific rays emanating from the earth, than it does those from the sun."—J. R. Mayer, on *Celestial Dynamics*.

To trace in their fullest extent all the conditions connected with solar radiation, it is necessary to measure its effects in different latitudes; to follow its changing influence through the different months of the year; to estimate its progress during the several hours of the day; to trace its power on the varied tribes of vegetation; and to discover, under all the diversified circumstances, its maximum force. In like manner, in order to trace the laws which regulate terrestrial radiation, the extent of its power must be traced in different latitudes; its influence upon plains and mountains compared; and its effects in the several months measured.

TERRESTRIAL RADIATON.

In the absence of the sun the heat acquired by the earth during the day, is radiated away through the atmosphere into space. Nebulous and invisible aqueous vapour equally exert a retarding influence upon this process; whence, in a great measure, result the equable climates of insular and maritime regions, since these are favourably situated as regards humidity. On the contrary, inland countries, especially those characterised by the absence of great rivers or lakes, are naturally dry; hence, having the conditions for freest action of radiation, their climates are extreme.

Diurnal radiation, when powerful, scorches the skin and withers tender plants. Nocturnal radiation kills tender plants by the rapid abstraction of heat from all their surfaces which are exposed to a clear sky; and causes the formation of dew and hoar-frost.

In the British Isles, vegetation, says Daniell, is liable to be affected at night, from the influence of radiation, by a temperature below the freezing point of water, ten months in the year; and even in the two months July and August, the only exceptions, the radiant thermometer sometimes falls to 35°. Mr. Lowe observed, near Nottingham, on 2nd June, 1864, the temperature on the grass as low as 23°·3. Many potatoes, which were two feet high, were killed, as well as french and kidney beans; even the young shoots of the ash were destroyed.

The instrument usually employed for measuring the effect of nocturnal radiation is a registering alcohol thermometer. It should be protected by a glass shield through which the graduations, etched upon its stem, may be read; instead of being mounted in a wooden or metallic frame. The thermometric spirit should be quite colourless. This instrument is usually placed, with its bulb exposed to the sky, just above the points of grass, the stem being supported horizontally. Whether or not a better instrument, or a better plan of using this, can be devised, is a matter which should engage the serious attention of meteorologists. For without doubt observations indicative of the radiation from grass are not strictly comparable. Not only does the radiating power of grass vary with the species, it varies even with the luxuriance and length of the blades—while the situation, whether on a plane or sloping surface, and the nature of the subjacent soil must also influence the process differently in different places.

The apparatus used by Daniell for observations on nocturnal radiation consisted of a spirit thermometer placed in the focus of a parabolic reflector, resting on the grass, or soil, and exposed to the sky. The reflector was made of copper, and plated with silver; its diameter was six inches, and the length of its focus 1¼ inches. The thermometer was passed through a collar in its side, the scale being kept on the outside. The reflector was placed upon a foot with a ball and socket joint, that enabled it to turn in any direction.

"Dr. Wollaston was the first to expose a concave metallic mirror turned upwards to the free air, with a thermometer placed in its focus; and he found that the thermometer indicated a lower temperature after being thus exposed for a short time."

Daniell considered this the most accurate mode of measuring terrestrial radiation. "The radiant thermometer is so completely insulated by the reflector from the counter radiation of surrounding bodies that it may be applied with equal effect in any situation, where the aspect of the sky is very limited. Even in the streets of London, where the radiation of an exposed thermometer is nearly neutralized, and the utmost effect never exceeds two or three degrees, that of the thermometer, guarded by the reflector, is wholly unimpeded. Experiments that are thus made, in whatever situation, are strictly comparable, provided they are screened from any strong action of the wind."

Mr. Glaisher's experiments on nocturnal radiation did not lead him to speak so favourably of the parabolic reflector. Thus—to quote his memoir on this subject in the *Philosophical Transactions*, 1847,—he writes:—

"The thermometer whose bulb is in the focus of the mirror when the clouds are low remains stationary, and frequently reads higher than any other thermometer however placed; the reason seems to be that the heat radiated from the cloud, being received on the reflector and reflected to the bulb in the focus, exceeds that radiated from the thermometer."

Mr. Glaisher has shown that, under the same state of sky, the excess of the reading of the thermometer in air above that in the reflector is the same very nearly at all times of the year, but that the amount of the excess varies with every variation of the quantity of cloud.

The following table expresses the average excess, according to the state of the sky, of the readings of a self-registering thermometer placed in air at the height of 4 feet, and protected from radiation, above those of a similar thermometer placed in the focus of a metallic parabolic reflector, fully exposed to the sky.

Amount of cloud. (Scale 0 to 10).	Number of nights.	Mean excess.
10	321	2·74°
8	198	4·13
5	282	5·52
2	257	7·06
0	331	8·33

In all simultaneous observations it was found by Mr. Glaisher that the readings of the thermometer in the reflector had no advantage over the readings of one placed on lead. Thermometers which were from one to three inches from the top of grass generally read the same as that in the reflector. Sufficient consideration has, apparently, not been given by meteorologists as to the best substance to be employed as a radiating surface with a view of obtaining observations at different places which may be comparable. It would seem that glass or lead might be found to possess advantages over grass.

The relative depressing power of various substances laid on grass, to that of long grass, assigned by Mr. Glaisher, from a mean of all his observations, are given below, long grass being taken as a standard:—

SUBSTANCE.	From simultaneous observations.		From registering thermometer.	
	Radiating power.	No. of observations.	Radiating power.	No. of observations.
Hare skin - - - - -	1316	70	...	...
Rabbit skin - - - -	1240	59	...	...
Raw white wool - - -	1222	357	1280	276
Unwrought white cotton wool -	1085	228	1167	28
Long grass - - - -	1000	479	1000	467
Flannel - - - - -	871	29	...	...
In focus of parabolic reflector -	858	464	962	716
Glass - - - - -	864	163	...	...
Sheet copper - - - -	839	172	...	...
Blackened tin - - - -	770	104	844	84
Sheet lead - - - - -	757	224	1000	72
Jet black lamb's wool - -	741	33	...	...
Sheet zinc - - - -	681	213	...	...
White tin - - - - -	657	112	677	94
Snow - - - - -	657	Few.	...	...
Sheet iron - - - - -	642	161	...	...
Slate - - - - -	573	100	...	...

The following table gives the results obtained by Mr. Glaisher by exposing glass thermometers at different heights above the surface of a grass field:—

	Radiating power.	No. of observations
Long grass	1,000	479
Above the points of the grass,		
One inch	671	226
Two inches	570	9
Three „	477	89
Six „	282	165
One foot	129	212
Two feet	86	199
Four „	69	87
Six „	52	18
Eight „	17	96

To the experience of Daniell and Glaisher we will add the opinion of Sir J. Herschel, as expressed in his "*Meteorology.*"

"When a thermometer is exposed with its bulb in the focus of a concave silver hemisphere* or paraboloid, highly polished both internally and externally, and deep enough, when exposed with the concavity upwards, to cut off from the thermometer all view of the earth, and as it were to continue the sky beneath it, it can only receive heat by condensation† and radiation from the air, and from the condensation of moisture. A thermometer so exposed under a clear sky, and shaded from direct sunshine, always marks several degrees below the temperature of the air, and its depression affords a rude measure (of the statical kind, and open to all the objections to which that kind of measure is open‡) of the facility for the escape of heat by radiation afforded under the circumstances of exposure."

Terrestrial radiation is even a more difficult phenomenon for the traveller to estimate than solar radiation, the danger of exposing instruments at night being always great in wild countries. Avoiding radiation from surrounding objects is very difficult, especially in wooded countries. The thermometer and metallic reflector appear to be suitable for the varying conditions in which a traveller would make observations on noctural radiation.

THE SPECTRUM OF AQUEOUS VAPOUR.

The optical study of aqueous vapour shows that it possesses an elective power of absorption for light, or in other words, that this vapour produces dark lines and bands in the

* "The figure is absolutely of no importance. A paraboloid is generally used, for no reason that we can perceive but to increase the cost."

† Query, *conduction?* ‡ See page 140.

spectrum of a luminous ray which traverses a sufficient thickness of it.

Certain constant lines in the solar spectrum were found to be incessantly variable in their intensity with the height of the sun, that is to say, with the thickness of the earth's atmosphere traversed by the sun's rays. An investigation of the spectrum on a high mountain, the Faulhorn, proved that these lines grew weaker in proportion to the height ascended, that is, in proportion as the solar rays traversed a smaller thickness of the terrestrial atmosphere. Thus it became obvious that the earth's atmosphere affected the characteristics of the spectrum. It was found that the earth's atmosphere produces in the red, orange, and yellow of the spectrum, a system of lines ten times as numerous as the solar lines of these regions; on the contrary, in the green, the blue, and the violet, the lines of solar origin predominate.

An attentive study of the solar spectrum led to the idea that this action of the atmosphere is attributable to the aqueous vapour dissolved in the earth's atmosphere, if not wholly, at least to a very important part. Comparisons, followed for a long time, on the solar light, during various seasons of the year, showed very clearly that for the same heights of the sun certain lines of the spectrum were more pronounced as the dew-point was higher. On the Faulhorn, on extremely dry days, the lines in question disappear almost entirely from the spectrum.

There could thus be little doubt as to the action of aqueous vapour; yet it was necessary, seeing the importance of the result, to submit this point of theory to a direct verification, by investigating the modifications which a well-defined beam of light of known composition experienced by the fact of its passage through a tube of sufficient length containing only aqueous vapour.

An iron tube 37 metres in length was mounted; it was placed in a wooden box of the same length, filled with well dried wooden shavings—an arrangement which avoids all appreciable loss of heat. The vapour was furnished by a moveable steam-engine of six-horse power, and the light by a lamp of six jets, placed in the direction of the axis of the tube. The experiments confirmed in the completest manner, what the study of the solar spectrum had already indicated. In one experiment (August 3rd, 1866), in which the tube, well freed from air, was full of vapour at the pressure of seven atmospheres, the spectrum was observed with five dark bands, two of which, well marked, spread from D to A (Fraunhofer), and recalled the solar spectrum seen in the same instrument towards sunset.

From the first comparisons made between the spectrum of aqueous vapour and that of solar light, the group A of Fraunhofer, B (in great part at least), the group C, and two groups between C and D are due to the action of the aqueous vapour of the atmosphere.

This experiment has further given an interesting result. The spectrum of the transmitted light was seen to be very dark in the most refrangible part, while it was brilliant in the regions of red and of yellow. Thus, although aqueous vapour energetically absorbs certain red and yellow rays, it is very transparent for most of these rays, while it acts in a general manner on the more refrangible rays. It follows from this that aqueous vapour would be of an orange colour by transmission, and the redder the greater the thickness through which it acts.

The results will have to be verified and established with the greatest care. If it be finally demonstrated, we shall find in it an explanation of the red colour, so variable in its tints, but always observed at sunset as well as at sunrise.

The consequences of this discovery of the spectrum of aqueous vapour will be overlooked by no one. We are finally agreed as to the origin of a considerable portion of the rays of the solar spectrum; and the knowledge of these rays will allow us to investigate the higher regions of our atmosphere as regards their humidity—regions which at present are inaccessible to our means of investigation. But it is more especially in astronomy that the results will be interesting to develope.

This article has been abridged from a memoir by the discoverer M. Janssen, printed in *Comptes Rendus*, August 13th, 1866, a translation of which is given in the *Philosophical Magazine*, No. 216.

[We have endeavoured in this appendix to place before the reader, in a popular manner, the leading features and general conclusions of the latest researches into the functions and properties of the aqueous vapour of the earth's atmosphere. We have been careful to use, as nearly as possible, the words of the illustrious experimentalists and writers themselves, and our object throughout has been to give only such general information as may well lead the reader to the original sources, the works and memoirs which we have always distinctly indicated. The paths of physical science, in which Tyndall and Janssen have been the pioneers, will assuredly lead to important and fertile fields of inquiry. The applications of our knowledge regarding the relation of aqueous vapour and radiant heat, and the spectrum of aqueous vapour, will most probably be of the utmost importance in giving an impetus and a reform to the pursuit of meteorology. Day by day new lights and new ideas will be evolved, and what is here recounted will soon need supplementing.]

D. Lane, Printer, Strand, London, W.C.

International Exhibition, 1862.

AWARDED FOR CASELLA'S PATENT

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COMBINED WITH ACCURACY AND EXCELLENCE

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ADDRESS.

THE Meteorological information and instruction contained in the first Edition of this List having been much approved of, has induced the publication of this second, and, it is hoped, improved edition.

To Directors and Managers of large Observatories, or other public Institutions, it presents a practical selection of STANDARD INSTRUMENTS, embracing every recent improvement, whilst to Amateurs desirous of taking reliable observations, it gives a faithful means of enabling them to select, and thus commence at the outset with suitable Instruments, the indications of which will bear the strictest test.

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For Clinical and general Medical Investigations, Thermometers and other Instruments have also been arranged by L. C., including an accurate means of measuring the ventilation of Hospitals, an economic Microscope for Medical Students, &c., &c.

A brief List has also been prepared of L. CASELLA'S Economic Series of METEOROLOGICAL INSTRUMENTS, for Beginners, Gardening and Horticultural purposes, for which he has also been honoured with a First Class Certificate from the Royal Horticultural Society. The great object being to adopt each Instrument in the best manner, to the particular purpose for which it is required.

23, HATTON GARDEN, LONDON, E.C.

January 1st, 1865.

STANDARD

METEOROLOGICAL INSTRUMENTS,

MANUFACTURED BY

L. CASELLA.

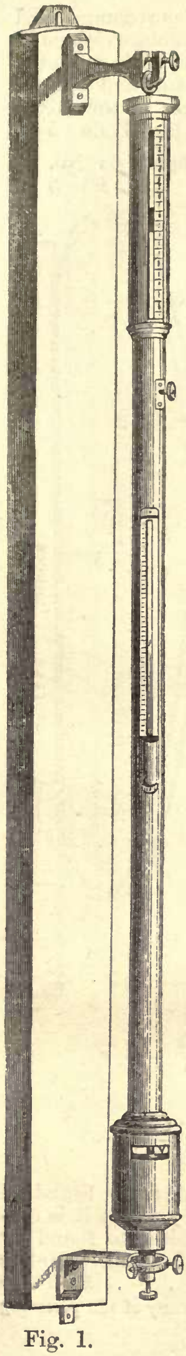


Fig. 1.

BAROMETERS.

1. **Standard Barometer** (*fig.* 1), on Fortin's principle, with brass frame, glass cistern, and fixed ivory point. The mercury boiled in the tube, which is 0·4 in. internal diameter. The mercury in the cistern is adjustable to the zero, or ivory point, by means of a thumb-screw acting upon a flexible base. The vernier, with rack adjustment, reads to the 1-500th part of an inch, or, by estimation, to ·001 inch. In front of the barometer a thermometer is attached, in contact with the tube, with divisions etched on the stem. For facility of reading, a moveable jointed reflector is attached to, and moves with, the vernier. The instrument is suspended from a bracket at the top of the mahogany board, so as to ensure perfect perpendicularity. At the bottom of the board is a socket, with clamping screws for steadying the barometer when an observation is made. The instrument is so mounted, that it can be turned at pleasure to any source of light. £8 10 0

(If with Millimetre Scale, 10s. 6d. extra.)

2. **Standard Barometer of extra large size,** tube 0·75 in. internal diameter, arranged for observatories, £20 0 0

Instructions.—The Barometer may be placed in any convenient room, where it is not near a fire, or exposed to the sun's rays. It should be in a good light, with the scale about five feet from the ground, so that the zero point in the cistern, and the vernier on the scale may be easily seen. First, hang the board on the wall, then insert the lower part of the cistern through the bottom bracket, and suspend the instrument as in *Fig* 1. Next turn the small thumb screws of the lower bracket to fix it in the perfectly vertical position in which it hangs. When the Barometer is thus placed unturn the thumb screw till the mercury falls in the cistern to the level of the ivory point, and *then* unturn the small iron screw on the top of the cistern to admit the air more freely.

To Read the Barometer.—First read the attached thermometer, and adjust the mercury by means of the thumb screw so that it barely touches the ivory point in the cistern, which, with its reflection, will then appear as a double cone—this point is the zero of the scale; the height of the column is then taken by adjusting the lower edge of the vernier, so that it shall exactly form a tangent to the convex surface of the mercury in the tube, just excluding the light by keeping the eye in the same plane with the back and front lower edges of the vernier. Every care should also be used to avoid influencing the temperature whilst making the observation.

3. **Standard Barometer**—on the Kew principle, in which the graduations of scale are arranged to compensate for the rise and fall of mercury in the cistern, whereby the reading from a point in the cistern is obviated. The mounting, &c., being the same in every respect as No. 1. . . . . £5 15 0

. . . Capable of greater delicacy than any other. . . . *The most convenient form of all* Maximum Thermometers."

In "Practical Meteorology," by John Drew, Ph. D., F.R.A.S., &c., we find "Its construction is simple and its indications sure, as those can testify who have used it."

An arrangement of Phillip's Maximum Thermometer has just been effected by L. Casella for clinical and physiological investigations. See Aitkins's "Science and Practice of Medicine;" 3rd Edition. London: 1864.

Fig. 13.

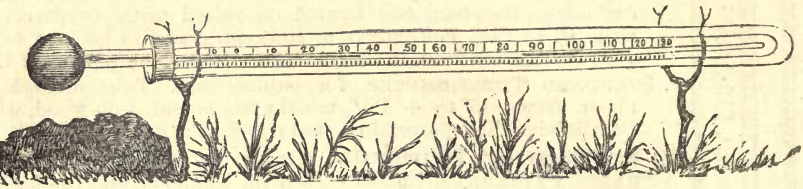


Fig. 14.

Directions for Using the Maximum Thermometer.—Suspend the instrument by means of the brass loop attached to the back, let the other end rest upon a projecting nail (*a*) to keep it horizontal, as shown in fig. 11; disengage and lower this end to allow the detached portion of mercury to approach the rest, which it will do within about one quarter of a degree. On an increase of temperature, the mercury will rise as in an ordinary thermometer, and continue to do so as long as the heat increases, propelling the detached portion to whatever extreme the heat may attain. On a decrease of temperature, the mercury will contract and recede in the usual manner, leaving the detached portion to indicate the highest temperature, which it does at the end furthest from the bulb.

CASELLA'S PATENT MERCURIAL MINIMUM THERMOMETERS.

15. **Casella's Mercurial Minimum Thermometer,** on porcelain scale, with hard-wood back, and divided on the stem (*fig.* 15) £1 15 0

16. **Casella's Mercurial Minimum Thermometer,** for Terrestrial Radiation, divided and figured on the stem, and enclosed in glass cylinder for protection, like fig. 13 . . . . £1 15 0

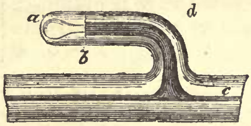
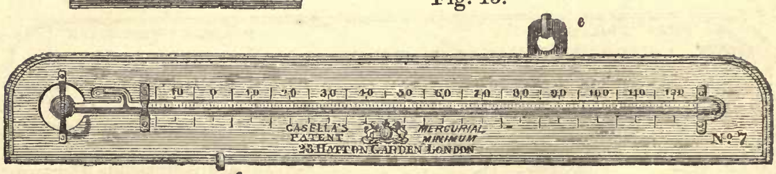


Fig. 15.



This is the only practical Mercurial Minimum Thermometer hitherto invented. Mercury is the only fluid employed in this instrument. The bulb and column are made of the same size as in the Maximum Thermometers; and cold is thus registered under precisely the same conditions as heat; no steel or other index is employed; whilst the annoyance arising from sluggishness, evaporisation, and breakage of the column in the spirit minimum, is entirely avoided. The general form is shown in *fig.* 15; *d* being a tube with large bore, at the end of which a flat glass diaphragm is formed by the abrupt junction of the small chamber (*a b*), the inlet to which at *b* is larger than the bore of the indicating tube. The result of this is that, having set the thermometer, the contracting force of the mercury in cooling withdraws the fluid in the indicating stem only, whilst on its expanding with heat, the long column does not move, the increased bulk of mercury finding an easier passage through the larger bore into the small pear-shaped chamber attached.

Directions for Using the Mercurial Minimum Thermometer.—Place the instrument in a horizontal position, with the back plate (*e*) suspended on a nail, and the lower part supported on a hook (*f*). The bulb end may now be raised or lowered, causing the mercury to flow slowly until the bent part (*d*) is full and the chamber (*a b*) QUITE EMPTY. At this point the flow of mercury in the long stem of the tube is arrested by adhesion to the diaphragm (*b*), and indicates the exact temperature of the bulb, or air, at the time. On an increase of heat the mercury will expand into the small chamber (*a b*); and a return of cold will cause its recession from this chamber only, until it reaches the diaphragm (*b*) to which it adheres. Any further diminution of heat withdraws the mercury down the bore to whatever degree the cold may attain, where it remains until further withdrawn by increased cold, or till re-set for future observation. When out of use, or after transit, it may be that raising the bulb may not at first cause the mercury to flow from the small chamber as above; in such case a slight tap or jerk with the hand on the opposite end with the bulb up, will readily cause it to do so.

THE VALUE OF THIS INSTRUMENT IS BEST SHOWN BY THE FOLLOWING TESTIMONIALS:—

From SIR HENRY JAMES, R.E., F.R.S., *Director of the Ordnance Survey and Topographical Depot of the War Department, Author of "Instructions for taking Meterological Observations," with Tables, Notes, &c.*:—"I have great pleasure in stating that, after having had one of your mercurial minimum thermometers carefully observed and registered at this office, and one at Southampton for the last three months, and during a period in which we have had a great range of temperature, I have found it to act perfectly, and never once to get out of order. I therefore think you have achieved a great success, and hope you will receive its reward."

B. STEWART, Esq., *Director of the Kew Observatory, in his Description of the instrument before the meeting of the British Association for* 1862, said:—"Before bringing this instrument to the notice of this association I have carefully tested its action at the Observatory, and find its indications in every way satisfactory."

In the proceedings of the BRITISH METEOROLOGICAL SOCIETY for November, 1862, "Dr. Thompson, Vice President, F.R.S., again directed the attention of the members to this instrument, in the use of which he had derived the highest satisfaction ever since its invention."

From H. STORKS EATON, Esq., M.A., *of the British Meteorological Society*:—"*Little Bridy, Dorset.* Sir,—I have found your Minimum Thermometer much more sensitive than the ordinary alcoholic minimum. It *is* particularly well adapted for determining the amount of *Terrestrial* RADIATION, and has not once failed during the twelve months I have had it in use."

From F. F. TUCKETT, Esq., *of Bristol*:—"The idea really is a beautiful one, and admirably carried out. The Meteorological world are indeed indebted to you and your son for this excellent invention."

From T. LAWRENCE, Esq., *Medical Staff, Mooltan, Punjaub, India*:—"Your Mercurial Minimum Thermometer continues to work admirably. It seems to me the only instrument adapted for minimum registration in this climate."

STANDARD MINIMUM THERMOMETERS.

17. **Minimum Thermometer,** filled with pure alcohol, for ordinary registration, engine divided on the stem, and mounted to correspond with the Standard Maximum No. 10, *(fig.* 17*)* . . , ... £0 18 6
18. MINIMUM THERMOMETER, on porcelain scale, divided and fitted as above, and mounted to correspond with Maximum No. 11 . . . . £0 18 6
19. MINIMUM THERMOMETER, for terrestrial radiation, divided and figured on the stem, which is enclosed in a glass cylinder for protection, *(fig.* 19) £0 17 6
20. **Maximum and Minimum Thermometer** combined, in neat pocket mahogany case, for travellers, as expressly designed for the Livingstone Expedition. The tubes are divided on the stem, and may also be used as strictly standard instruments . . . , ... £2 0 0
21. Ditto, ditto, of smaller size, as designed for the Alpine Club £1 5 0

Fig. 17.

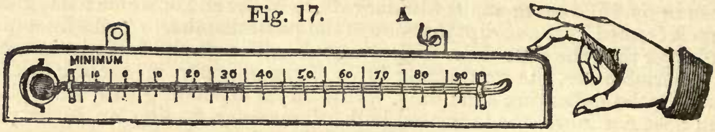


Fig. 19.

Directions for Using the Spirit Minimum Thermometer.—Suspend the top end by a hook (A) *fig.* 10, the bulb end resting on a projecting nail (B), and about half an inch lower than the opposite end. To set the index, press gently on the suspended end, which will raise the other and bring the index to its place, *i.e.*, the edge of the fluid inside. As the temperature decreases the spirit will recede and take the index back with it; but, on an increase of temperature, the spirit will advance, leaving the index to mark whatever extreme of cold may have occurred. If, in transit, the index is shaken out of the spirits, or the spirits separated, all is easily rectified by a swing or two of the arm, holding the bulb downwards, and then letting the thermometer hang with the bulb down about ten minutes, to allow the fluid to settle and become perfectly united.

HYGROMETERS.

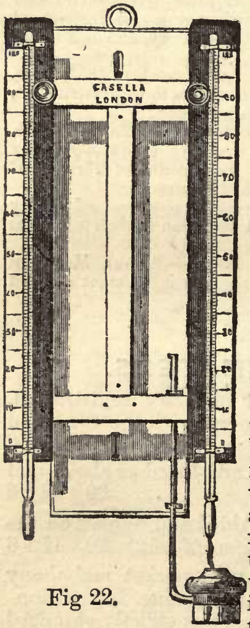


Fig 22.

22. **Mason's Hygrometer** (wet and dry bulb), metal scales, mounted on mahogany board for suspension. The Thermometers are divided on the stem, and the figures burnt in on Casella's indelible porcelain slips at the side, as supplied by L. Casella to the Members of the British Meteorological Society *(Fig.* 22) £2 2 0

23. Ditto ditto, as above, with porcelain scales £1 15 0

24. **Mason's Hygrometer,** extra size, with expanded graduations, the tubes being fifteen inches long, and divided to 0°.2, mounted, &c., as *Fig* 22 £3 3 0

The wet and dry bulb, or MASON'S HYGROMETER, consists of two thermometers placed parallel, about four inches apart, with their graduations as nearly as possible identical. The bulb of one is covered with thin muslin, from which trail a few threads of lamp cotton; these, being first wetted, are passed into a small attached vessel of water, two or three inches distant, and the bulb thus kept continually moist, causes this thermometer to indicate the temperature of evaporation, whilst the dry bulb thermometer shows the temperature of the air. From the readings of the dry and wet thermometers, the dew-point may be obtained by means of the accompanying tables. During frost, however, when the capillary action of the cotton is stopped, it is best to cut off the projecting threads, leaving the bulb covered; and when an observation is required the bulb should be wetted, by means of a camels-hair brush, with water as near 32° as possible. In such cases it is not necessary to remove the ice from the bulb, but merely remove the drop which first forms from the water, the temperature will then speedily settle so as to indicate the point of evaporation.

The following table will greatly assist in familiarizing the inexperienced with the value of the Hygrometer indications:—

DRY BULB.	DIFFERENCES OF DRY AND WET BULBS.						DRY BULB.	DIFFERENCES OF DRY AND WET BULBS.					
	2°	4°	6°	8°	10°	12°		2°	4°	6°	8°	10°	12°
34°	79	63	—	—	—	—	58°	87	76	66	57	49	—
36	82	66	53	—	—	—	60	88	76	68	58	50	43
38	83	68	56	45	—	—	62	88	77	67	58	50	44
40	84	70	58	47	—	—	64	88	77	67	59	51	45
42	84	71	59	49	—	—	66	88	78	68	60	52	45
44	85	72	60	50	—	—	68	88	78	68	60	52	46
46	86	73	61	51	—	—	70	88	78	69	61	53	47
48	86	73	62	52	44	—	72	89	79	69	61	54	48
50	86	74	63	53	45	—	74	89	79	70	62	55	48
52	86	74	64	54	46	—	76	89	79	71	63	55	49
54	86	74	64	55	47	—	78	89	79	71	63	56	50
56	87	75	65	56	48	—	80	90	80	71	63	56	50

Representing by 100 the total quantity of vapour which the air at any temperature can hold in suspension, the percentage of vapor actually present at any time may be found in the table, opposite the temperature of the dry bulb thermometer, and under the difference of the dry and wet bulb readings. The percentage corresponding to intermediate degrees and differences, not given in the table, can be readily inferred. Thus, air temperature being 63°, and difference 5°, the humidity of the air would be about 72°.

In the British Isles the Hygrometer very seldom shows a greater difference than 12°, hence it is presumed that the table will be of much practical utility. Below freezing, 32°, the observations of the Hygrometer are uncertain; but then they are not of so much importance, since the state of the air is known to be dry.

If the difference between the dry and the wet bulb readings be taken from the reading of the wet bulb, the remainder will be the temperature of the dew-point, very nearly Glaisher's factors should be employed for greater accuracy. They are given in most works on meteorology.

25. **Daniel's Hygrometer**; the thermometers engine divided on the stems, with ether test, complete in mahogany case (*fig.* 25) £3 3 0

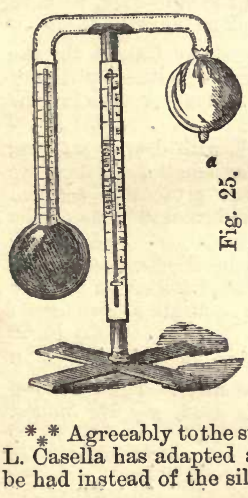


Fig. 25.

This elegant instrument consists chiefly of a bent glass tube, with two balls—a black one containing ether, the stem of which encloses a sensitive thermometer with the bulb placed in the centre of the ball, and a white one covered with thin muslin, the interior of the tube being thoroughly deprived of air.

Directions for Using Daniel's Hygrometer.—Turn the instrument so that by applying the warm hand to the covered bulb all the ether goes into the black bulb, then place it upright as shown in the sketch, and pour ether upon the muslin enveloping the white ball (*a*) *fig.* 13, and when sufficient cold is produced by evaporation of the ether from the black ball to condense the moisture of the atmosphere upon its surface, in the form of a ring just below the centre, the internal thermometer will show the exact temperature at which the deposition of dew takes place, which is called the dew-point.

26. **Regnault's Condensing Dew-Point Hygrometer** (Casella's Improved), with ether bottle, &c., complete in mahogany case (*fig.* 26) . £4 4 0

*** Agreeably to the suggestions of Colonel Sykes, F.R.S., and Dr. Miller, F.R.S., L. Casella has adapted a black glass bottle, with silver neck and tube, which may be had instead of the silver bottle, or extra, at an additional charge of 20s. 6d.

Although Mason's Hygrometer has for some time been in general use, yet Regnault's is now much employed for taking direct observations of the dew-point. In its present portable form it consists essentially of two sensitive thermometers—one exposed to the action of the atmosphere, and the other to the influence of a current of air passing through ether. An important part of this instrument is the small polished silver bottle into which, through an ivory stopper, one of the thermometers is inserted. On one side, within the bottle, a small silver tube descends nearly to the bottom, the other end, passing outwards, is connected with a small flexible aspirating tube. Supporting the bottle is a hollow bent neck, connecting it with a stand, which is also hollow, by which the air freely escapes at the base.

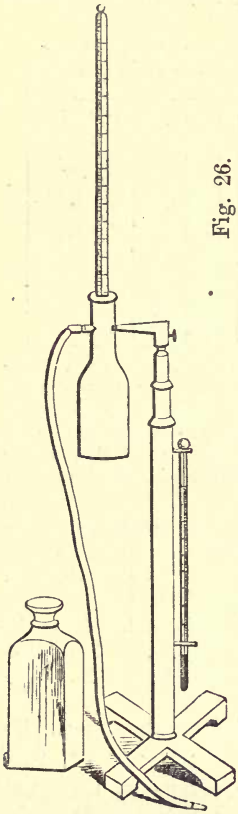


Fig. 26.

Directions for Using Regnault's Hygrometer.—Pour just as much ether into the silver cup as will cover the bulb, and insert the thermometer as shown in the drawing. On causing the air to bubble slowly through the ether, by breathing through the tube, the immersed thermometer will show a decline in the temperature; and when a film of moisture forms on the larger part of the silver bottle, the temperature at that instant indicates the dew-point. The observer should stand so as not to allow the breath or heat of the person to affect the instrument.

RAIN GAUGES.

27. **Rain Gauge,** as made for the Observatories of the War Department, of stout copper, height 10 inches, receiving surface 10 inches square, and funnel formed inside to prevent evaporation, with jar graduated to hundredths of an inch *(fig.* 27*)* £1 5 0

28. **Rain Gauge, Casella's,** pedestal form, 3 feet high, receiving surface 9 inches in diameter, made of stout copper japanned, with strong glass tube graduated to show 3 inches of rain in tenths and hundredths, with extra stop-cock for frosty weather. In this arrangement the rain is measured as it falls, being visible at all times in the glass tube, and is poured off by simply turning the stop-cock, without removing the gauge from its place *(fig.*28) £3 10 0

29. Rain Gauge (Dr. Livingstone's), as expressly arranged by Casella for the Zambesi Expedition; receiving surface 3 inches diameter, with small stout glass measure divided to hundredths of an inch, in leather case for the pocket £0 16 6

30. Rain Gauge, cylindrical form, of stout copper, 7-inch, with deep brass rim, and inside receiving pan and bottle, by which large or small quantities are measured without disturbing the gauge, and efficient protection secured against evaporation or frost, or of overflow during the heaviest rains of the tropics £3 10 0

31. Rain Gauge (Fig. 31), 5-inch diameter, with receiving-bottle, &c., as arranged by G. J. Symons, Esq., to meet the desire of the British Association in their recent grants to him for the purpose of obtaining an accurate knowledge of the rainfall of the British Isles £0 15 6

32. Ditto, ditto, of Copper, for durability £1 1 0

33. **Evaporating Dish or Gauge,** of copper, with wire gauze cover. The receiving surface of same diameter as the gauge with which it is used, say five inches, with graduated glass measure £0 15 6

34. Evaporating Gauge, as above, seven inches £0 18 6
Ditto, ditto, seven inches £1 5 0

The increasing importance attached to a knowledge of the quantity of water which falls on, or evaporates from, various localities, has caused the exercise of considerable judgment in the construction of instruments the best suited for this purpose. Extended experiments, with areas or receiving surfaces, varying from three to twenty inches in diameter, have led to the adoption of medium sizes for standard and general instruments.

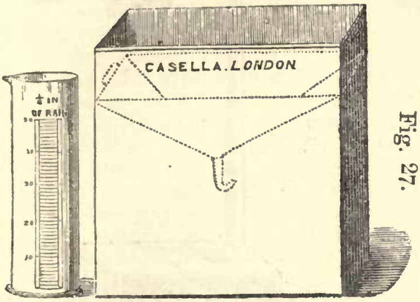


Fig. 27.

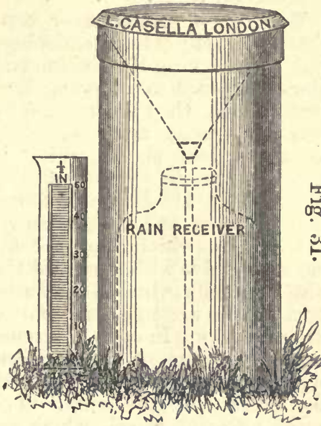


Fig. 31.

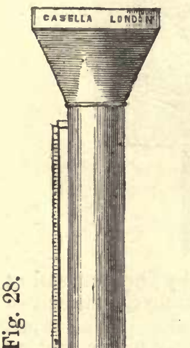


Fig. 28.

Instructions for Evaporating Gauge.

Nearly fill it with water, measured by the graduated glass measure, and place it out of doors freely exposed to the air. After exposure, again measure the water, and the difference between the first and second measurement shows the amount that has evaporated. Should rain have fallen, however, during the interval, the quantity equal to that collected in the adjoining rain gauge must first be deducted from the evaporator, the remainder—compared with the measured quantity put in—showing the amount that has evaporated. For districts which are subject to very heavy rainfall, an evaporating gauge with overflow pipe to meet any exigency may be had at a slight increase in the price—these, however, are not so easily used, and are not recommended except when obliged by such necessity.

ANEMOMETERS.

35. **Anemometer (Robinson's)** Small size, with four Index Wheels, registering successively 100, 1,000, 10,000, and 100,000 revolutions. In this arrangement the cups travel at the rate of one-third the wind's velocity, and each revolution represents 3.14 feet; thus $3.14 \times 3. = 9.42$ feet, being the distance travelled by the wind for each revolution. This again multiplied by the number of revolutions indicated on the dial, shows the distance the wind has travelled between one observation and another. The dials are read from right to left, and the amount indicated at the last observation is to be deducted from that shown on the dials at the time of the current observation . £3 3 0

36. **Robinson's Anemometer,** for Registering the Velocity of the Wind in miles and tenths of miles, greatly improved and modified, for registering to 500 miles, (*fig.* 33) described by Sir H. James, F.R.S., in his Book of Instructions for Meteorological Observations, page 29 . . . . £4 4 0

37. Robinson's Anemometer, as above, to register to 5000 miles £5 5 0

This gauge has an extra dial to record to 5,000 miles, each division representing 500 miles, as shown by the first dial.

38. Lind's Anemometer £2 2

This instrument though ingenious in theory and improved by Sir Snow Harris is not recommended, and must fall into total disuse as the self-recording principal of Dr. Robinson becomes more known.

39. **Anemometer for Sanitary Investigations,** for showing the exact amount of draft or current of air passing in any part of a room down to one foot per second £4 0 0

Robinson's Anemometer consists essentially of four hemispherical cups, having their diametral planes exposed to a passing current of air; they are carried by four horizontal arms attached to a vertical shaft, which is caused to rotate by the velocity of the wind. Dr. Robinson found that the cups, and, consequently, the axis to which they are attached, revolve with one third the wind's velocity, which is measured by a simple arrangement of two wheels working in endless screws, and, by means of two indices, shows, on inspection of the dial, the velocity of the wind. The outer or front wheel, which revolves once for every five miles, is furnished with two graduated circles, the interior circle being subdivided to miles and tenths of miles, whilst the outer circle is divided into one hundred parts, each being equivalent to five miles, so that it measures 500 miles of wind. The stationary index at the top of the dial marks on the inner circle the number of miles (UNDER FIVE), and tenths, that the wind may have traversed, in addition to the miles shown by the traversing index, which revolves with the dial and indicates on the outer circle the transit of every five miles. This anemometer is rendered extremely portable by the arms, which carry the cups, being made to take off. When in use, it may be screwed on a shaft, or ordinary piece of iron pipe, which accompanies it, and fixed in any desirable position.

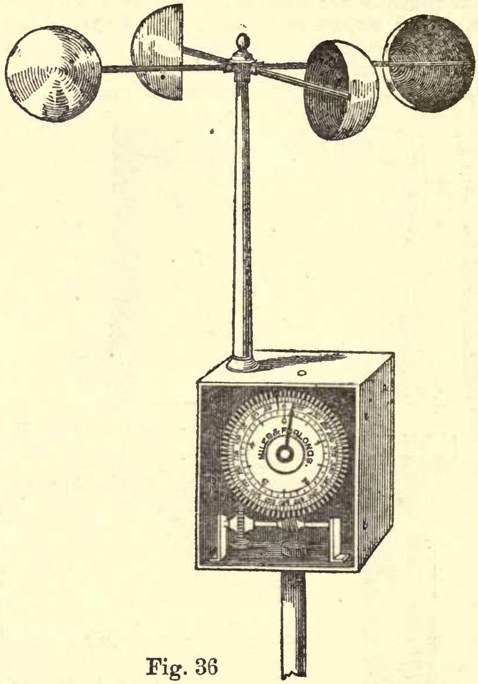


Fig. 36

The mode of reading the instrument is best explained by example. Suppose the stationary pointer reads 1 mile 2½ tenths, *i.e.*, 1·25 miles, and the traversing index reads 25 on the outer circle, the reading is the sum, or 26·25 miles. This reading must be subtracted from the subsequent reading to ascertain the velocity of the wind in the interval. Suppose the second reading, say next morning, to be 24·05 miles; subtracting the former reading, 26·25, the difference, 214·25 miles, is the distance traversed by the wind in the interval. The observations should be recorded as *read off*, and a second column of the register should be provided for daily differences, or other interval as the observer may select. When the traversing index has passed the 500, the observer should make a * in the register before the next reading. Thus, suppose the reading yesterday was 387 miles, and to-day * 154 miles: here it is evident that the index has passed beyond the 500; so we place the * before it, and, in taking the difference, treat it as 654, which gives 267 miles of wind. Every star in the register counts as 500 miles of wind. Suppose the mean daily velocity of the wind for any interval, say one month, be required, all that is necessary is to take the reading of the instrument on the last day, add to it 500 for every * which appears in the register during the interval, subtract the reading on the first day, and divide the remainder by the number of days.

40. **Enlarged Anemometer,** for Harbours and Public Observatories, on the Kew principle, with clock-work arrangement for registering the time of the velocity and direction of the wind. The action of the Kew Anemometer on this principle has not once ceased during the nine years that it has been in use. £65 0 0

41. Ditto ditto if made to register in a lower room of the building £75 0 0

42. **Hypsometrical Apparatus,** for measuring mountain heights by the vapour of boiling water. The extended improvements effected in this instrument of late by L. Casella are such as to render it by far the most portable means we have of measuring great elevations. The thermometer, strong, with small bulb, is divided and figured on the stem, and sheltered when in use by a double telescope chamber into which it is placed to any required depth through a loose piece of india-rubber which rests on the top; the chamber being filled with vapour from the boiling water beneath. The inner chamber, and tube thus completely enveloped, the vapour descends in the outer chamber and escapes by the outlet. By this means the mercury both in the bulb and stem is immersed in pure vapour, whatever kind of water may be employed. (*fig.* 42.) The portable leather case (*fig,* 42*) contains the whole when packed for travelling. Price, with one thermometer, divided to 0°.1 . £4 15 0

43. **Hypsometrical Apparatus.**—The complete success attending the above arrangement has induced L. Casella to arrange a still smaller apparatus on the same principle, which has been much used by the members of the Alpine Club. It may be carried with perfect ease in the trowsers' pocket, and, by those a little experienced in its use, is often preferred, for its simplicity and certainty, to the Mountain Barometer. With one thermometer divided to 0°.5 . £2 10 0

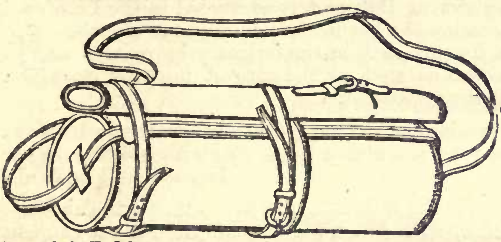


Fig. 42*.

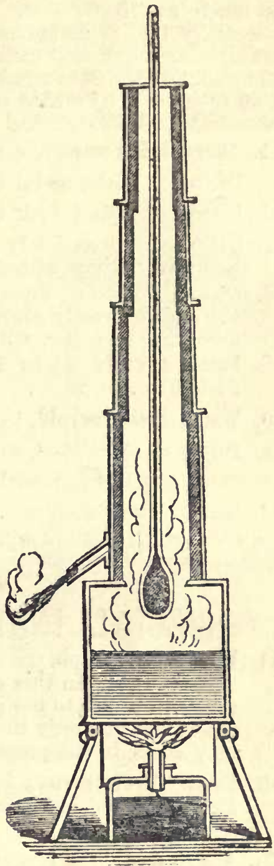
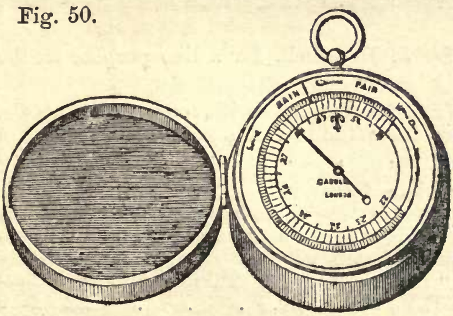


Fig. 42.

Casella's Tables, with instructions for using the Hypsometrical Apparatus £0 1 0

ANEROID BAROMETERS.

Fig. 50.



The Aneroid, though not an independent instrument, or capable of the same permanent precision as a standard mercurial barometer, is yet probably the most portable and sensitive means we possess of measuring ordinary variations in atmospheric pressure and foretelling changes in the weather, either on land or at sea, whilst for measuring moderate heights it is most valuable and convenient. A well made ordinary instrument will measure approximately the heights of mountains up to 5,000 or 6,000 feet, the height of a house, or even the difference in height between one room and another, and are now made by L. Casella to measure as much as 15,000 feet. This ingenious and elegant instrument depends essentially for its action on the compressibility by the atmosphere of a thin flat circular corrugated metallic box, from the interior of which the air has been removed, and the flat surfaces of which are held in a state of tension or separation from each other by means of springs. The varying weight of the air gains or loses on this resistance, and is indicated by means of the hand upon the dial.

44. **Aneroid Barometer,** card dial, ordinary size, in case . . £2 10 0

45. Ditto ditto, metal dial £3 0 0

46. Ditto, ditto, with thermometer £3 3 0

47. Ditto, ditto, finely divided, with extra compensation for temperature, with or without thermometer £4 0 0

48. Ditto, ditto, ditto, with jewelled movements, highly finished; the scale expressly adapted for measuring heights up to 15,000 feet, according to order, with improved revolving index . £6 to £8 8 0

49. **Small Aneroid**, $2\frac{1}{2}$ by $1\frac{1}{4}$ inches, for the pocket, graduated from 24 to 31 . . . . £3 0 0

50. **Watch Size Aneroid,** to show up to 10,000 feet of elevation (*fig.* 56) £4 0 0

51. Ditto, to 15,000 feet, with thermometer £4 10 0

52. Ditto, to 3,000 feet, with expanded scale of graduation . . £5 0 0

53. Ditto, ditto, in silver . . . . , £5 10 0

*** The Watch Size and Altitude Aneroids above-named are thoroughly compensated for temperature, have the latest improved works, and improved revolving indices.

SPECIAL INSTRUMENTS MADE TO ORDER.

54. **King's Barograph,** or self-registering Barometer, as erected in the Liverpool Observatory. In this construction the barometric column may be caused to range from five to ten inches for each inch in the ordinary barometer, and to record continuously the smallest as well as the largest fluctuations which may occur in the pressure of the atmosphere.

55. **Earth Thermometer,** for ascertaining the temperature below the soil, or the heat developed in hay-stacks, pine and melon pits, &c., with pointed copper tube £0 18s. 6d., and upwards.

56. **Æthrioscope,** the invention of Sir John Leslie, for ascertaining the absolute intensity of terrestrial radiation, with which instrument time is an element of an observation.

57. **Pyrheliometer,** the invention of M. Poullet, for ascertaining the absolute heating effect of the sun's rays, or "solar radiation," time being an element of observation.

58. **Actinometer,** the invention of Sir J. Herschell, for a like purpose to the above.

59. **Anemoscope,** or Portable Wind Vane, for travellers, with compass, bar needle, &c., showing the direct course of the Wind to half a point of the compass (*fig.* 59) . . . . , ... £2 2 0

60. **Thermometer Stands.**—Of the various forms in use, that designed by Sir Henry James, for the use of the Royal Engineers in all climates, seems in general to be the most convenient. It is easily made and fixed, and shelters the instruments alike from the sun and rain, whilst it admits of a perfectly free circulation of air. See "Instructions for Taking Meteorological Observations," by Sir H. James, R.E., F.R.S., &c. £2 2 0

MISCELLANEOUS INSTRUMENTS.

61. **Ozonometer,** consisting of strips of paper prepared with iodide of potassium and starch. The papers are to be suspended so as to be exposed to the free access of air, but sheltered from wet and the direct rays of the sun: when affected by ozone, they become tinged, the intensity of which is measured by a graduated scale of twelve tints, which accompanies the ozonometer £0 6 6

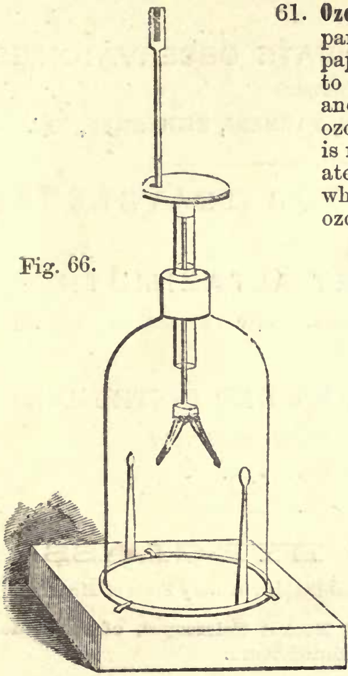


Fig. 66.

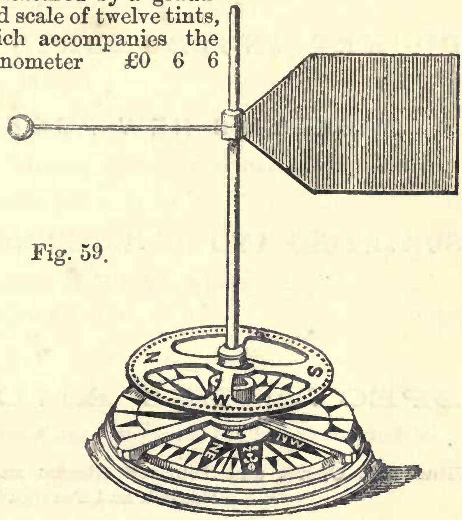


Fig. 59.

62. **Sedan's Ozonometer,** latest construction, with scale 0 to 21 . £0 8 6
63. OZONE CAGE of fine wire gauze, as recommended by Sir James Clark £0 15 6
64. OZONE CAGE, small size, for travellers £0 15 6
65. **Casella's Forms for Registering Ozonometer Indications** for one year £0 1 6
66. **Electrometer** (Gold Leaf), for atmospheric electricity, with 2 ft. brass collecting rod, clip for lighted fusee, and improved mode of insulation, with stand and book containing spare gold leaves (*fig.* 66) £0 18 6

To replace the gold leaves, unscrew the top brass plate which supports them, and moisten with the breath the flat brass piece to which they are to be attached; press this gently down on one end of a gold leaf, and the opposite side on another in the same way.

67. **Casella's Forms for Registering Meteorological Observations,** with concise remarks and instructions, in twelve monthly forms for one year £0 4 0
68. **Portable Meteorological Register and Note Book,** with Weather Diagrams, Tables, and Instructions. (Post Free) £0 3 0

STANDARD WORKS ON METEOROLOGY.

	£	s.	d.
1. "Meteorology," by Sir J. Herschell	0	5	0
2. "The Laws of Storms," by Professor Dove	0	10	6
3. "The Weather Book," by Admiral FitzRoy	0	16	0
4. "Practical Meteorology," by John Drew	0	5	0
5. "Meteorological and Physical Tables," by Professor Guyot	0	16	0
6. "Hygrometrical Tables," by J. Glaisher	0	2	6
7. "Instructions for taking Meteorological Observations," by Colonel Sir H. James	0	7	6
8. Board of Trade "Barometer Manual:" or, How to Foretell Weather, by Admiral FitzRoy, F.R.S. Post free	0	1	0
9. Thompson's "Introduction to Meteorology,"	0	12	6
10. Piddington's "Horn Book of Storms,"	0	16	6